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Vol. 216 AUGUST, 1933 No. 2

AGRICULTURE AND SCIENCE.*

BY

HON. HENRY A. WALLACE,

Secretary of Agriculture.

I count it a considerable honor to be the guest of The Franklin Institute on this occasion. Whether, in inviting me to address you, you distinguish between my activities as Secretary of Agriculture and my activities as a scientist, I have no means of knowing; but I hope I have thus far escaped the sort of fame enjoyed by a certain gentleman who is known both as an economist and a journalist, and who is referred to by economists as a highly successful journalist, and by journalists as a highly successful economist.

I would not have you carry the parallel too far, however. Some such dual rôle may be forced upon scientists, before we are out of our present economic disorder.

I doubt if scientists have considered, as much as they should, the impact of the present economic situation upon science. Our present impasse seems to me to shed new light on past contributions of science, and to impose new burdens on the science of the present and the future. Or, to put it another way, our present economic difficulty specifically challenges science to defend itself against alleged excesses of the past, and asks for a pledge to contribute with more certainty to human welfare in the future.

* Presented at a special meeting held Tuesday, May 9, 1933.

(Note.—The Franklin Institute is not responsible for the statements and opinions advanced by contributors to the JOURNAL.)

Perhaps I can best explain what I have in mind by referring specifically to the scientific work of the Department of Agriculture. Here is a department in which scientific research is not merely the Department's biggest job, but the foundation of all its other jobs. The Department's agricultural extension service, coöperating with the States, is clearly designed to help farmers apply the results of research. In a host of regulatory and service activities—from food and drug law enforcement to the building of highways—scientific research provides the essential foundation upon which we build. Practical though all this can be shown to be; financially useful though it may be to industry and agriculture alike—still it does not protect the basic scientific work of the Department from the barbs of the uninformed.

For many months organized groups have waged war on scientific research in government. They have objected that it costs money which is not directly returned to their pockets. They have reasoned that we are in this economic impasse in part by virtue of the excessive efficiency of scientific production. They would virtually eliminate scientific research from the Government, and they make no bones about saying so. And if from the Government, why not from the publicly supported colleges, and all other public institutions?

Mind you, I am quite ready to agree that when the income of citizens and of the Government is as drastically reduced as it is at present, we have no choice but to reduce all expenditures of Government, and scientific work must shoulder its share of the reduction. Conceivably, a searching examination may reveal dead wood here and there; some of our less obviously important scientific activities can be slowed down in deference to the emergency; but that is very different from a heedless elimination of governmental activity because it is scientific, or of scientific activity because it is governmental.

Suppose we consider our agricultural research much as a business man would. The total value of the corn, wheat, and cotton crops of the United States is normally about four billion dollars. Around these three basic crops the entire agriculture of the United States revolves. If a private corporation had an annual output this valuable, how much

would it be spending for scientific research to reduce production costs, discover new uses, and the like? Probably anywhere from forty million to four hundred million dollars.

Does that seem fantastic to you? An officer of the National Research Council, in a letter to Dr. A. F. Woods of our Department last summer, reported the results of a query sent to several hundred industrial corporations. In 309 replies representing 19 classes of industries, it was revealed that most of the corporations spent from one to ten per cent. of their total sales on scientific research. About one in every six corporations, of those reporting, spent more than five per cent. of their sales on research, and one in every fourteen spent more than 10 per cent. of their sales. Thus the range—forty million to four hundred million dollars—when computed on the basis of a total output of four billion dollars.

You may ask how Government's expenditures for research compare with industry's. The Federal Government, for this four-billion-dollar crop of corn, wheat, and cotton, has been in the habit of spending around three million dollars annually, for scientific research. That is a very small fraction of one per cent. Even on the tragically reduced values of these crops in 1932, Federal expenditures for research amounted to less than two-tenths of 1 per cent. Similarly with dairy products, the farm value of which in 1932 amounted to more than a billion and a quarter dollars, and on which Federal research expenditures amounted to about a million dollars, or 0.08 per cent. By the term Federal expenditures, it should be explained, we include not only what the Department of Agriculture spends directly in its own laboratories and in the field, but what the Congress appropriates, through the Department of Agriculture, for expenditure by the State experiment stations.

The estimated gross farm income from swine production amounted to a billion and a half dollars in 1929. The Department's expenditures for research in swine production and marketing in that year amounted to less than $125,000. About two-fifths of that money went for research in feeding and breeding, in swine management in general. Another third, or thereabouts, went for research on diseases—on parasites of swine, on hog cholera, and on pathological

investigations of various kinds. For studies in marketing and standardization, the Department spent $20,000.

It would be interesting to compare expenditures of various years with gross farm incomes, but I fear such a study would fail to show any correlation between total sales, as an industrialist might state it, and research expenditures.

The three-million-dollar expenditure for research on corn, cotton, and wheat may be reflected upon in the light of the fact that these great crops occupy a total of nearly 200 million acres of land, and absorb the energies of more than six billion hours of man labor annually. The scientific work of the Department of Agriculture and the State experiment stations during the past 30 years has undoubtedly made its imprint upon even this vast acreage and expenditure of human labor. Though a relatively small investment, the expenditures for research by the Federal and State governments have undoubtedly made it possible for the farmers of the United States to produce the present volume of corn, wheat, and cotton with a billion hours less man-labor annually.

This is a tremendous increase in efficiency. It has been of great benefit to a few farmers—those who were first able to utilize it—and to all the consumers of food and fiber in the United States. In fact, it has been of greater benefit to them than to the rank and file of the farmers.

Hence the attacks upon science from many sources, and the necessity for science to defend itself, first, against such attacks, and second, against the consequences of its own successes. If I may paraphrase a famous remark, we have somehow to make science safe for democracy.

What I mean by that is this: That science has magnificently enabled mankind to conquer its first great problem —that of producing enough to go around; but that science, having created abundance, has now to help men learn to live with abundance. Having conquered seemingly unconquerable physical obstacles, science has now to help mankind conquer social and economic obstacles. Unless mankind can conquer these new obstacles, the former successes of science will seem worse than futile. The future of civilization, as well as of science, is involved.

In the recently published "Recent Social Trends" the problem has been stated thus:

"Americans have been well schooled in a theory of economics which starts with production, a theory born of the uncounted centuries of recurring famines and shortages. This theory in America received tacit governmental sanction in the efforts of the agricultural extension service constantly to increase per-acre yields through better technic.

"But now what was once held impossible has happened. Production has outstripped consumption and is caught in the web of the economics of a period that never visioned such a possibility. Want and suffering are rampant in a land of plenty.

"It seems right to ask therefore whether the serviceable social science of today and tomorrow must not consider these things."

I would not minimize, even momentarily, the past accomplishments of science, in government or out. I do not see how any intelligent person can hear some of the complaints registered against research without desiring to jump at once to the defense. Certain business groups, a few highly articulate Chamber of Commerce people, have done a great injustice to the scientific work of the Government by inferring that considerable sums of money have been spent to investigate how far a flea could jump, and to look into the love-life of the frog.

I am unable to find any account of a Government project dealing with the love-life of a frog. It is true that someone wrote into the Department of Agriculture to inquire how far a flea could jump, and that one of our entomologists, after a brief search through the scientific literature on fleas, was able to make a suitable reply. The publicist who ridiculed the Department for this jumped to a variety of inaccurate conclusions, one of them being that special and expensive research was involved, and another being that fleas, from whatever aspect, are screamingly funny. They seem to be in the same category with spinach.

As many of you know, research on fleas—including a knowledge of how far they can jump—has been fairly important for the human race. For through this research it was learned, back in 1906, that the dread bubonic plague was carried from rats to man by fleas. The knowledge of how

far a flea could actually jump was of considerable importance in fighting outbreaks of the plague shortly thereafter.

I don't believe I should have been ashamed to have had the Department of Agriculture discover that knowledge, amusing though the very subject of fleas may be to a publicist with an axe to grind.

In any event, I am not disposed to apologize for a certain amount of Government money spent in pure research. On the whole, I am inclined to think that more money should be spent in this way rather than less. Let me give an illustration coming out of my own experience. It happens that 20 years ago this summer, I did my first experimenting with the in-breeding of corn and was led to do so by the work in pure science done along this line by Dr. G. H. Shull of the Carnegie Institution, and Dr. E. M. East of the Connecticut experiment station. Dr. Shull especially had no thought whatever of bringing his in-breeding work in corn to a practical conclusion. More than 20 years ago he dropped his corn breeding experiments and turned to the breeding of evening primroses and the weed known as shepherd's purse. Yet I may say that the purely scientific work started by Dr. G. H. Shull 28 years ago can easily make it possible for us within the next 10 years to produce our present supply of corn on 80 million acres instead of 100 million.

I say this advisedly and with full knowledge, because last year in Iowa more than 1,000 farmers grew corn produced by this method which originated in the pure science of Shull and East. As an average of a thousand weighed up comparisons it was found that the corn produced by crossing in-bred strains out-yielded the farmers' own corn by 10 bushels an acre.

The Federal and State scientists engaged in this particular branch of research know from a practical point of view that their discoveries have just begun. They also know that from the point of view of pure science, there are strange vistas opening up in many directions. The corn geneticists of the United States Department of Agriculture and the State experiment stations have developed several thousand in-bred strains of corn which when properly combined have in them the ultimate potentiality of saving the corn farmers of

the United States a billion hours of man labor annually. A saving of this magnitude properly distributed through the right kind of economic machinery can be of enormous help to all the people of the United States.

I have no patience with those who claim that the present surplus of farm products means that we should stop our efforts at improved agricultural efficiency. What we need is not less science in farming, but more science in economics. We need economic machinery corresponding in its precision, its power and its delicacy of adjustment to our scientific machinery. Science has no doubt made the surplus possible, but science is not responsible for our failure to distribute the fruits of labor equitably. We must charge that failure squarely up to organized society, and to government.

In some respects, scientists must shoulder the blame for some of our present economic distress. Relatively few scientists, I fear, have a well developed social point of view. It may be that specialization has forced them into the cloister, out of touch with the surge of economic and social problems. It may be that the Darwinian theory of the survival of the fittest has so impressed them that they have permitted it to color their views on economics as well as on biology. For there is a very direct relationship between the survival of the fittest theory and the theory of laissez-faire which has so long dominated our economic thinking. Or again, it may be that scientists have had to applaud any consequence of profit-motive, if they were to pursue their research. Generally removed from any desire for great wealth, scientists may not always have realized the problems daily arising from an inequitable distribution of wealth.

Whatever the basis, I believe the time has come for men of science to consider both the effect of current economic problems on science, and the contributions that science may be able to make to the solution of those problems.

Perhaps the star of the social scientist—who seems to specialize, so to speak, in a social point of view—is rising. I have often wondered what the character of modern science would be if the early scientists had been biologists, for instance, like Pasteur, rather than mathematicians and physicists, like Newton and Galileo. At any rate, it is true

that the social and biological sciences have lagged far behind the physical sciences.

I want to pay a tribute to those scientists who deal with life. While I have the greatest respect for the physicists, the astronomers and the engineers, I have an even greater respect for the scientists dealing with living organisms. I am sure, as a result of my own continuous contact with the corn plant during the past 30 years, that both the pure scientists and the practical scientists dealing with corn are on the verge of unusual discoveries. This same situation doubtless exists with the scientists dealing with other plant and animal organisms. I have the feeling that it is time for the human race to devote its attention more definitely toward the life side rather than the mechanical side of things. As more and more scientists work with the problem of bringing together the superior germ plasm of the different plant and animal organisms, a social consciousness will be built up which will find an eventual expression in the search for superior human germ plasm. These things, of course, will develop slowly, but to my mind in this direction we shall find some of the most hopeful activities of the human race. We may become eventually disillusioned by our efforts at mechanical progress, but if we look deeply into the life side of things, we will find endless vistas unfolding which, as they express themselves in the social world, are not so likely to be disillusioning as the activities which are built on the inorganic sciences.

I am speaking to you about these things because of the fact that a very high percentage of the scientists dealing with genetics and biology are employed either by the United States Department of Agriculture or the State experiment stations. I suspect that 90 per cent. of their efforts may seem foolish from a narrowly practical point of view. They are, therefore, peculiarly subject to attack from the unthinking criticism of so-called practical business men.

I fully agree that scientists should be subject to some of the same vicissitudes as the rest of humanity in these troublous times. They should be willing to take their share of salary cuts and reduction in activities. Some of these men are engaged in projects which should be stopped. It is certainly necessary in times like these to re-examine scientific projects

in order to discover which ones might well be discontinued. But I think it would be a serious mistake completely to destroy, as some people apparently wish to do, such scientific work as that done by the Department of Agriculture.

Industry is equipped to do its own scientific work because of the corporate form of organization. In agriculture, however, not one farmer in 10,000 is in position to do genuine scientific work. There is an excuse and a need for State and Federal Governments to do scientific work for agriculture, where there is no such excuse and need for them to do similar work for industry. Furthermore, it seems a strange thing that we applaud the expenditure of money designed to increase the profits of a private industry, while at the same time we question the expenditure of money designed to return a profit to society as a whole.

In defending the scientific work of the Department of Agriculture, I am not making a plea for the farmers. Much of our research expenditures are of direct interest only to industry. All of them benefit city standards of living far more than rural incomes. Indeed, I believe the standard of living of the next generation will be definitely lower, especially in the cities, if the scientific work of the Department of Agriculture is destroyed at this time merely to save ten or fifteen million dollars a year.

I want the standard of living, in city and country alike, constantly improved. But we shall never achieve that desideratum unless we learn how to distribute what we have produced, as well as we have learned to produce it. Science has done the first job, and done it magnificently; now let it turn to the second and infinitely more difficult one. I feel assured that the challenge appeals to your imagination, and to your craving for social justice, as much as it does to mine.

Clarence Henry Eckles, 1875–1933.—W. W. SWEET (*Sigma Xi Q.*, 1933, XXI, 87–91, 95) has written a biographical sketch of the late Dr. Eckles, with an account of his contributions to the advancement of the dairy industry in the United States. During his connection with the University of Missouri, from 1901 to 1919, the annual output of dairy products in that state grew from a comparatively small figure to approximately $30,000,000. From 1919 to 1933, he played an equally important part in the development of the dairy industry of Minnesota, through his connection with the University of Minnesota. In his own name and in collaboration with others, he published three books, 109 scientific papers, and numerous popular articles. His researches were in the numerous fields, which are closely associated with the dairy industry, both biological and chemical, such as nutrition, vitamins, chemical composition of milk and butter, biophysics, bacteriology, and physiology. His former students fill responsible positions in research, teaching, and administration in the dairy industry.

J. S. H.

Color Reaction of Sucrose.—HARRY W. RAYBIN (*Jour. Am. Chem. Soc.*, 1933, LV, 2603–2604) describes a new color reaction of sucrose with diazouracil. The sucrose (40 to 50 milligrams) is dissolved in 5 cc. of 0.05 normal sodium hydroxide solution. Then 7 to 10 milligrams of diazouracil are added, a cork is inserted, and the container is shaken until solution is complete. The solution is kept cold. A blue-green color develops within a few minutes; and the addition of a solution containing magnesium cations produces a stable blue precipitate. Raffinose is the only other carbohydrate which has been found to respond to this reaction; it yields a similar but fainter color. The test was applied to 35 additional carbohydrates and closely related compounds; none gave the characteristic blue-green color, although several produced a yellow or red-brown color.

J. S. H.

Protein Nature of Enzymes.—DAVID GLICK AND C. G. KING (*Jour. Am. Chem. Soc.*, 1933, LV, 2445–2449) present evidence that the lipase or fat-hydrolyzing enzyme of the pancreas belongs to the group of proteins known as globulins. It is soluble in a 10 per cent. solution of sodium chloride, and is precipitated quantitatively by saturation with magnesium sulphate.

J. S. H.

VARIATIONS IN THE SMALL-ION CONTENT OF THE ATMOSPHERE AND THEIR CAUSES.

BY

G. R. WAIT,

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In a recent issue of the JOURNAL OF THE FRANKLIN INSTITUTE, Koller [1] discusses the question whether the number of small ions in the atmosphere at a given locality has a seasonal as well as a daily variation. He states, among other things, that the number present is greater during the day than at night and is greater during the summer than during the winter. Recent work by the Department of Terrestrial Magnetism of the Carnegie Institution of Washington indicates this statement is open to some question, consideration being given the atmospheric-electric data accumulated by the Department at several different localities over sufficiently long periods to permit statistical analysis. Thus, in addition to the data taken aboard the *Carnegie* over the Atlantic, Pacific, and Indian oceans, there are the measurements of the electrical conductivity of the atmosphere (ionic content not being observed) made at Watheroo in Western Australia, Huancayo in Peru, Tucson in Arizona (in coöperation with the United States Coast and Geodetic Survey), and Washington in the District of Columbia. For the last station, conductivity of one sign only is recorded for a given time, but positive and negative conductivities have been recorded alternately since January 1930, while for the other stations simultaneous records of the positive and negative conductivity are available.

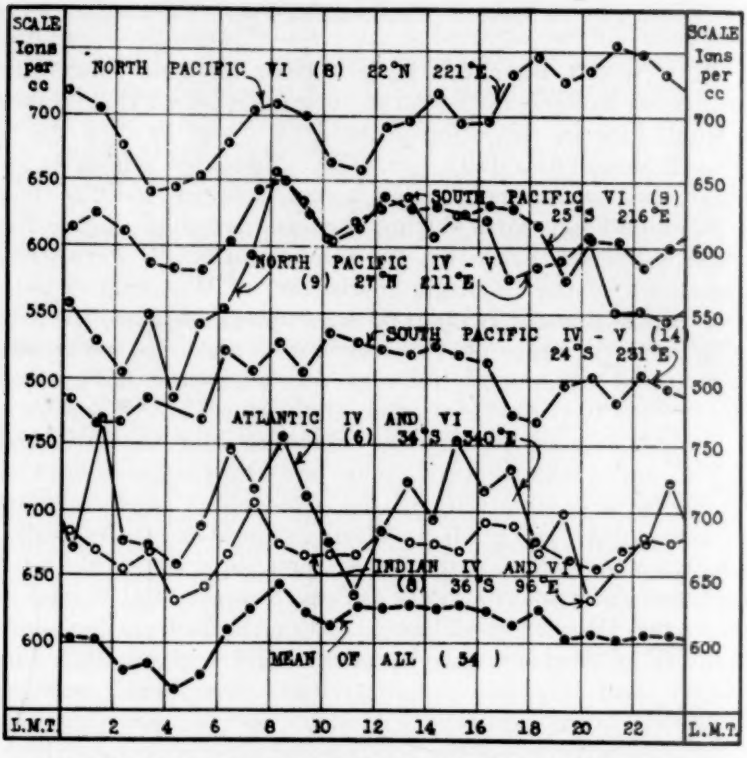
During cruises IV and VI (1916–21) of the *Carnegie* [2] there were obtained six complete diurnal-variation series of positive ionic content of the atmosphere made over the Atlantic, eight over the Indian, and 40 over the Pacific (see

[1] J. FRANK. INST., 214, 543–568 (1932).

[2] Researches of the Department of Terrestrial Magnetism, Carnegie Inst. Wash. Pub. 175, 5, 407–408 (1926).

Fig. 1). The mean of all 54 diurnal-variation series led Mauchly[2] to say "From the foregoing it appears one is warranted in expecting the ionic numbers over the ocean to be, in general, above the average value during the day and below the average during the night." Mauchly did not point out,

FIG. 1.



Diurnal variation of positive-ion content of the atmosphere from observations on the *Carnegie*, 1915–21.

however, that the average value for the day is only about five per cent greater than the average value for the night. In view of our present knowledge of the subject, this is hardly of sufficient magnitude to permit saying with any definiteness that the value during the day is greater than that during the night. This is emphasized when one considers the great

variability in ionic content as shown by the scatter in the diurnal-variation curves making up the mean curve in Fig. 1.

To investigate further the difference between the day and night values of the small-ion content of the atmosphere, the measurements of the conductivity of the atmosphere obtained at the four widely separated stations have been examined. Some question may be raised as to the justification for using values of conductivity instead of ionic content. The conductivity depends, of course, upon ionic content, ionic charge, and ionic mobility. The electronic charge per ion is generally accepted as being unity; the mobility, though perhaps variable, is not likely to vary greatly from day to night. On such a basis, variations in conductivity may be regarded as resulting largely from variations in ionic content. In Table 1 are the monthly mean values of the total conductivity

TABLE 1.

Mean Values of Total Conductivity During the Day Compared with Those During the Night at Observatories of the Carnegie Institution of Washington.

Observatory.	Latitude.	Year.	Total Conductivity = Positive + Negative Conductivity in 10^{-4} E.S.U.			
			January.		July.	
			Day.	Night.	Day.	Night.
Watheroo.....	30.3S	1931	3.2	3.0	4.3	4.2
Huancayo.....	12.0S	1931	8.1	13.6	4.4	10.5
Tucson.....	32.2N	1931	4.0	4.9	3.4	4.5
Washington....	39.0N	1930	0.17	0.17	0.32	0.38

(positive plus negative) at the Watheroo, Huancayo, and Tucson observatories for January and July 1931, and at the Washington observatory for January and July 1930. The average values for the day—that is, between sunrise and sunset—and those for the night show practically no difference for Watheroo and Washington, whereas at Huancayo and Tucson the values at night are greater than those during the day.

To determine to what extent the values in summer of the small-ion content of the atmosphere may differ from values in winter, the ionic-content data from cruises IV and VI of

the *Carnegie*[3] have been examined. Table 2 gives the *Carnegie* data arranged according to season of the year instead

TABLE 2.

Mean values of Positive and Negative Small-ion Content During the Summer Compared with Those During the Winter, Over the Oceans as Determined Aboard the Carnegie During Cruises IV and VI.*

Season.	Cruise IV.			Cruise VI.		
	Range of Latitude.	Small-ion Content.		Range of Latitude.	Small-ion Content.	
		Positive.	Negative.		Positive.	Negative.
	° °	*ions/c.c.*	*ions/c.c.*	° °	*ions/c.c.*	*ions/c.c.*
Summer	60N–40N	661 (6)	499 (5)	60N–40N	— —	— —
	40N–20N	541 (13)	490 (13)	40N–20N	730 (14)	582 (13)
	20N– 0N	605 (30)	550 (28)	20N– 0N	644 (26)	515 (24)
	0S–20S	502 (15)	344 (14)	0S–20S	551 (23)	451 (23)
	20S–40S	563 (19)	408 (19)	20S–40S	578 (26)	444 (25)
	40S–60S	551 (32)	457 (34)	40S–60S	494 (8)	346 (7)
Weighted means		565 —	464 —	———	604 —	476 —
Winter	60N–40N	— —	— —	60N–40N	— —	— —
	40N–20N	760 (5)	512 (5)	40N–20N	682 (21)	558 (21)
	20N– 0N	471 (8)	411 (9)	20N– 0N	421 (28)	363 (26)
	0S–20S	546 (3)	399 (4)	0S–20S	662 (25)	512 (19)
	20S–40S	642 (5)	432 (5)	20S–40S	738 (17)	618 (16)
	40S–60S	416 (2)	242 (2)	40S–60S	— —	— —
Weighted means		576 —	420 —	———	607 —	497 —

* Numbers in parentheses refer to the number of observations.

of according to the month and latitude as originally grouped by Mauchly. If these values be accepted as representative, it appears there is no consistent difference between the values in summer and in winter of ionic content over the ocean. The four observatories, above noted, are particularly well situated for making a study of this kind, since two lie south and two north of the equator. Thus, while the months of November, December, and January were grouped as "winter" for the two observatories north of the equator, they were grouped as "summer" at the observatories south of the equator; similarly, May, June, and July were grouped as "summer" for the two northern observatories and as "winter"

[3] Researches of the Department of Terrestrial Magnetism, Carnegie Inst. Wash. Pub. 175, 5, 411 (1926).

for the two southern observatories. At Huancayo, where thunder-storms occur so frequently in the afternoon, only the hourly values from midnight to 11 hours were used. All the 24 hourly values were used for the other observatories. The results are summarized in Table 3. From this it will be

TABLE 3.

Mean Values of Total Conductivity in 10^{-4} E.S.U. During the Summer Compared with Those During the Winter at Observatories of the Carnegie Institution of Washington.

Observatory.	Year.	Summer.	Winter.
Watheroo	1928	2.7	4.5
	1929	2.9	4.0
Huancayo	1930	11.5	9.7
	1931	10.5	10.6
Tucson	1930	5.2	4.5
	1931	4.4	3.8
Washington	1930	0.36	0.22

seen that during the summer the conductivity is appreciably greater than during the winter at Tucson and Washington, but that the reverse is the case at Watheroo. At Huancayo, the difference is not large. From these results it might be inferred that the absolute value depends not so much on the season of the year as on the month. It is believed, however, that this is not the case, for reasons given below.

The causes for possible changes in the small-ion content of the atmosphere from day to night and from summer to winter are of greater interest and of more fundamental importance than the variations themselves. The value of the small-ion content of the atmosphere at any locality depends upon the relative rates of production and removal. Practically no dependable experimental work has ever been done to show to what extent the rate of formation of small ions may vary at a given location through the day or season. The factors upon which the rate of removal of small ions from the atmosphere depends have been the subject of several investigations, but almost no attempts have been made to determine how these factors may vary through the day and year.

The rate of production of small ions, or, in other words,

the rate of ionization after equilibrium-conditions between production and removal have been established, may be represented to a first approximation by an equation originally suggested by McClelland and Kennedy,[4] namely,

$$q = \alpha n_+ n_- + 2\eta_2 n N,$$

where q represents the number of pairs of small ions produced per c.c. per second, n is the number of small ions of one sign per c.c., and N is the number of large ions per c.c. of sign opposite to n; α and η_2 are the coefficients of recombination having approximate values of 1.6×10^{-6} and 6×10^{-6} respectively.

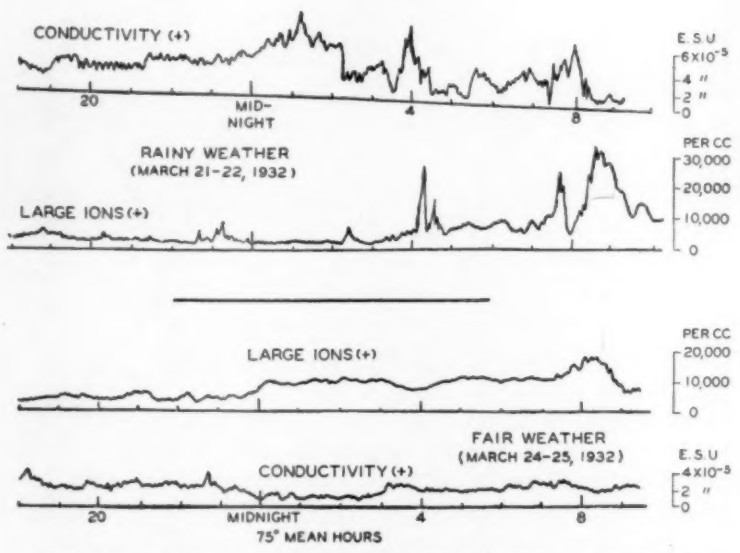
There is considerable evidence that the large ions come about through the charging up of condensation-nuclei that are ever present in the atmosphere. This evidence also indicates that, in general, the nuclei receive these electrical charges at the expense of the small ions. The condensation-nuclei are undoubtedly products of combustion; consequently in thickly settled communities, and especially in localities where there is considerable industrial activity, large number of these nuclei are to be found. In fact, in Washington, where the number of pairs of small ions averages only about 200 and the large ions are from 10 to 20 times as numerous, the value of the first term ($\alpha n_+ n_-$) of the above equation may be neglected in comparison with the value of the second term, and the equation would become $q = 2\eta_2 n N$ to a sufficient degree of approximation. It is thus seen that for a constant rate of production, the number of small ions in the atmosphere should vary inversely as the number of large ions. In Fig. 2 are reproduced published curves[5] showing representative records of electrical conductivity and large ions, obtained at Washington, D. C. To a large extent they exhibit a decided tendency towards a reciprocal relationship. When, however, the means of several days are taken, as in Fig. 3 (reproduced from the same article), this general reciprocal relationship is manifest for only a portion of the 24 hours. In general, the product of the number of small and large ions,

[4] *Proc. R. Irish Acad.*, A, 30, 89 (1912).

[5] G. R. Wait and O. W. Torreson, Slow Moving Ions in the Atmosphere. *Trans. Amer. Geophys. Union*, 13th Annual Meeting, 182–187 (1932).

which according to the equation of McClelland and Kennedy represents the rate of ionization, shows a definite and consistent diurnal variation. To what extent this may be attributable to an actual variation in the rate of ionization and to what extent to a misconception of the manner in which the small ions are removed from the atmosphere, can only be determined through experiment. Such experimental work has been in progress for some time at the Department of Terrestrial Magnetism. Any difference between the small-

FIG. 2.

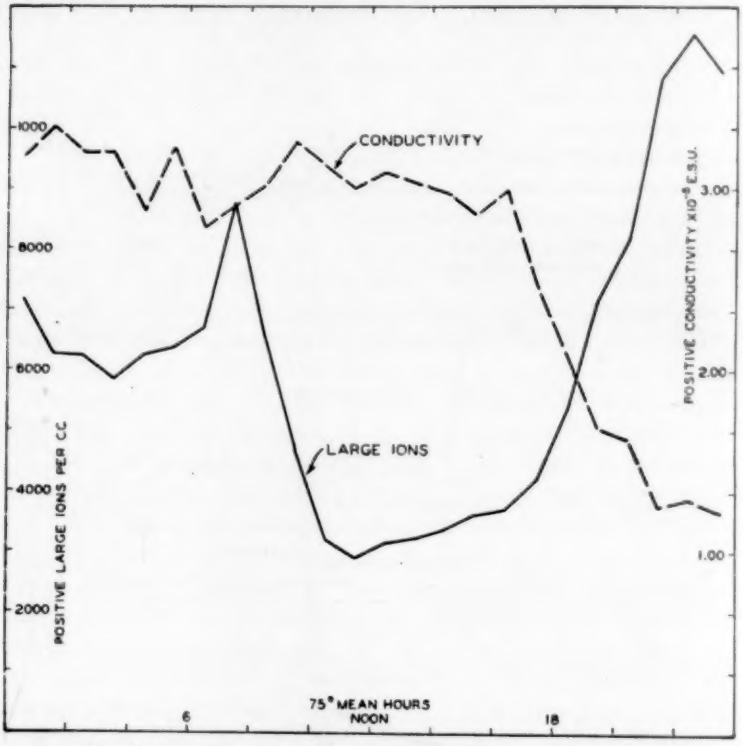


Typical photographic records showing reciprocal relationship between large ions and conductivity.

ion content of the atmosphere from day to night as indicated, for example, by Table 1 must be brought about through a change, as already pointed out, either in the rate of production or in the rate of removal of small ions, or in both. The number of large ions, which undoubtedly is one of the chief agencies causing the removal of the small ions, does have at Washington a definite diurnal variation (see Figs. 3 and 4). How representative these curves may be of other localities, we have as yet no way of knowing. In closely settled

communities, however, such as Washington, the number of large ions in the atmosphere during the winter greatly exceeds the number during the summer. As a consequence the small-ion content in such closely settled localities, where observations are most frequently made, shows a larger value during the summer than during the winter, thus giving rise

FIG. 3.

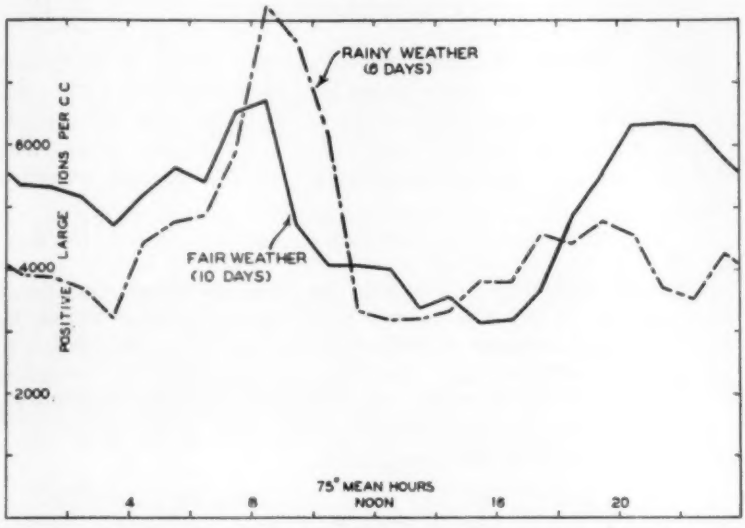


Comparison diurnal variation large ions and conductivity, Washington, D. C., March and April, 1932 (5 days).

to the general impression that this is always or at least generally the case. In contrast to this, the Watheroo data show a smaller value of the conductivity in summer than in winter (see Table 3). Watheroo is an isolated, subtropical station, not subjected to the introduction of nuclei from domestic and industrial fires. However, during the summer

numerous bush-fires burn in the vicinity, producing large numbers of condensation-nuclei which cause the conductivity to be smaller during that season of the year than during the winter.

FIG. 4.



Comparison diurnal variation number of positive large ions in fair weather and in rainy weather, Washington, D. C., March, 1932.

It may be concluded from the foregoing discussion that any variations in the small-ion content of the atmosphere must necessarily take place through a variation in the two primary agencies, namely, production and removal of the ions, and that the absolute value will not necessarily be dependent on the season of the year or time of day.

December 27, 1932.

Fluorates.—L. M. DENNIS AND E. G. ROCHOW (*Jour. Am. Chem. Soc.*, 1933, LV, 2431–2434) have produced an oxysalt of fluorine by electrolysis of a molten mixture of alkali hydroxides and fluorides. The silver salt of this oxyacid was prepared and examined, and apparently was silver fluorate, $AgFO_3$.

J. S. H.

Atomic Weight of Thallium.—GREGORY PAUL BAXTER AND JOSEPH SMITH THOMAS (*Jour. Am. Chem. Soc.*, 1933, LV, 2384–2387) have determined the atomic weight of thallium from the analysis of thallous chloride, and the measurement of the ratio of that compound to silver. The value obtained for the atomic weight is 204.40.

J. S. H.

Rhenium Sulphide.—According to J. H. MÜLLER AND W. A. LALANDE, JR. (*Jour. Am. Chem. Soc.*, 1933, LV, 2376–2378), the precipitation of rhenium sulphide from an ammoniacal solution by hydrogen sulphide is approximately quantitative. By repeated precipitation of the rhenium, this procedure may be used for the separation of rhenium from molybdenum, the latter remaining in solution as ammonium thiomolybdate.

J. S. H.

Reducing Action of X-Rays.—HUGO FRICKE AND EUGENE R. BROWNSCOMBE (*Jour. Am. Chem. Soc.*, 1933, LV, 2358–2363) report that X-rays reduce potassium dichromate. When 0.8 normal sulphuric acid is used as the solvent, 0.00000331 equivalents of the dichromate per 1000 cc. are reduced by a dosage of 1000 Roentgen units. The reduction is independent of the concentration of the dichromate and of the temperature within the range between 5° and 90° C. The reduction apparently is due to primary production of an activated form of hydrogen peroxide from the water, and is increased catalytically by the presence of traces of organic matter.

J. S. H.

THE SPACETIME SCALE AS A QUANTUM COÖRDINATE SYSTEM.

BY

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ABSTRACT.

In the Introduction the several branches of Physics are classified according to the sensory experience whereby the coincidence is recorded, thereby orienting the term "visual coincidence." The rôle of the clock mechanism in dynamical formulation is briefly discussed.

In Part I, an array of Michelson rotating mirror clocks, giving a postulated light-velocity effect, is used as a background for an empirical criterion for the synchronization of clocks.

In Part II, the empirical criterion for synchronization in question is formulated in such a way as to disclose the existence of an *absolute* kinematic quantum, capable of *macroscopic* test, contingent on the light-velocity effect postulated in Part I.

In Part III, the galilean spacetime scale is introduced as a kinematic "crystal" lattice whose gauge is the absolute kinematic quantum discussed in Part II. The question is raised whether in problems involving the ordering of sharp steady light images concentrated in lines or points, the images themselves may not form a three coördinate galilean or non-galilean spacetime lattice of synchronized clock-points, and are not more immediately described this way than by reference to the amorphous galilean or non-galilean four-coördinate reference systems. It is suggested that the quantum problem insofar as it treats of spectroscopic and crystallographic electromagnetic image patterns may be treated through an establishment of transformation equations between observed coincidences of a spacetime scale, as such, and the same observed coincidences referred to a uniform length scale.

INTRODUCTION.

Scale Definition. In a recent paper on "Physics and the Method of Coincidences," [1] the *scale* and *proto-scale* were defined in such a way as to lead to the following process definition of the recording of a physical datum, i.e. of a coincidence: When a scale and proto-scale are juxtaposed,

[1] Jour. Franklin Inst., 214, 443 (1932). The definition of a scale reading here given may be made more rigorous as occasion demands. Its present form will serve our immediate purposes.

and when one of the sensory organs is sufficiently stimulated (by an agency capable of stimulating the sensory organ in question) by this juxtaposition, so that the observer asserts that a distinguishing mark of the scale has at least one point in common with a distinguishing mark of the proto-scale; and, if, as a result of this realization the pre-assigned ordinal number (the relative scale quantum being purely conventional) of the scale distinguishing mark is recorded we have in our possession the record of a "pure" coincidence. When the proto-scale distinguishing mark stands between two scale distinguishing marks, we resort to the process called estimating. The record of a pure coincidence or a coincidence with its generally attendant estimate is a structure coördinate and can be simply [1] related to the coördinate of amorphous space.

Branches of Physics. We shall make frequent use of the term *visual coincidence.* The adjective "visual," laying stress, as it does, on the coincidence recorded through a visual sensory organ, seems by implication to allow for coincidences realized by sensory organs other than that of sight. We shall in this section attempt to give proper emphasis to visual coincidences by merely stating succinctly the possibility of other types of coincidences. This section will serve, therefore, more to orient that class of coincidences which are called "visual," than to present evidence for the utilitarian existence of other kinds of coincidences whose contemplation may seem to some, at first sight, a little bizarre.

By far the greater part of the data of Physics has been procured by way of visual coincidences, i.e. the assignment of a rational number to the superposition [2] of distinguishing marks of scales and proto-scales, the stimulating agent in this case being light energy. A physicist would say that a light image of the coincidence is propagated from the superposed scale-mark and proto-scale mark to the eye. A little reflection brings to light the fact that distinguishing marks are conceivable whose coincidences can impress themselves on other sensory organs, by way of beams of sound, for example,

[2] Since a *pure* coincidence record is the exception rather than the rule, the term superposition is taken in a generic sense to include the estimate. Thus the record of the datum has the form: [1] $(nB + \epsilon)$.

or by direct sense of touch. If this be true, it seems sequential to assert that to each of the five senses there corresponds a branch of Physics.

Thus we have:

1. The Physics of Visual Coincidences
2. The Physics of Aural Coincidences
3. The Physics of Olfactory Coincidences
4. The Physics of Gustatory Coincidences
5. The Physics of Tangible Coincidences,

corresponding to the sensory experiences of sight, hearing, smell, taste and touch respectively.

These branches of Physics may be further classified according to the experimental disposition whereby the coincidences in each are realized. The first class is that in which the coincidence image is said to be *propagated*. The second class results from immediate contact coincidences. Since experience shows that division into these classes (particularly where small propagational distances are concerned) are frequently not sharply defined, we shall call the propagational class, "virtual[3] contact coincidences," and the immediate contact class, "immediate contact coincidences." Thus, we have,

Virtual Contact Coincidences	Visual Coincidences Aural Coincidences Olfactory Coincidences
Immediate Contact Coincidences	Gustatory Coincidences Tangible Coincidences.

We make this classification because we may anticipate that many of the kinematic laws of Physics are similar within each classification.

[3] The concepts associated with the phrase "virtual contact" has taken on many forms, many shades of interpretation. See, e.g.: Lewis, *Proc. Nat. Acad. of Sciences*, **12**, 22 (1926). Slater, *Nature*, **116**, 278 (1925). Swann, *Science*, **61**, 425 (1925). L. de Broglie, *Phil. Mag.*, **47**, 446 (1924).

For the present we may assert that when two coincidences (each coincidence consisting of a proto-scale on a scale mark) are connected by a beam of light (or beam of sound), and if further at one of the coincidences there is a clock (see part III below) synchronized according to an empirical prescription with a clock at the other coincidence, then these two coincidences are in virtual contact. Thus the term virtual contact as used here implies possibilities for immediate experimental realization. The desirability of the term will become more evident in the sequel.

If there exist other senses (such as temperature sense) capable of recording coincidences of the kind described, then to each of these other senses there corresponds a branch of Physics.

It might be well to emphasize that: *Even though any or all of the senses may partake in the setting-up of apparatus, and the fixation of an effect, nevertheless, the branch of Physics the experimenter is working in, is determined by the sensory organ used to record the coincidence.* Thus the branches of Physics may be kept *distinct*, the criterion for classification being the *sensory organ* employed to record the coincidence.

We note in this connection that the fascinating experiments reported in the several volumes of the *Journal of the Acoustical Society of America*, seem to be paving an unequivocal way for a Physics of Aural Coincidences. The desideratum is to learn to read meters and clocks solely by ear, i.e. by sound images of coincidences of distinguishing marks of sound scales with sound emitting proto-scales. These distinguishing marks may take the form of delimited sound absorbers or emitters (analogous to black or white lines on a visual scale). At first the sounding scale will be coarse, but the introduction of aids to hearing (analogous to optical aids) will allow of closer packing of distinguishing marks. The outstanding problem seems to be twofold: (*a*) To make the distinguishing marks give such a sequence of unique sound images, one for each scale mark, that an ordinal number may be assigned to each scale mark, solely by ear; and (*b*) to construct a sound emitting clock mechanism, which will be recognized as such, without reference to any sensory organ other than the ear.

With such sound emitting scales and clocks numerous questions crowd in, demanding empirical answers. What, for example, will be the measured velocity of sound, determined by a sound scale and sound clock mechanism, solely by *listening* to sound coincidences? How will it depend on the relative motion of the observer with his clocks and scales and the sound bearing medium (at a given temperature and pressure). Will this velocity come out a constant under widely varying conditions? May we construct a relativity theory for aural coincidences?

These and kindred matters will be touched on in a future paper. The comparison of measurement relationships existing in one of the several branches of Physics with those of another branch cannot help but throw much light on the meaning of measurement in its relation to sensory calibration in general.

The Rôle of the Clock Mechanism in Dynamical Formulations. In the field of visual coincidences the agency whereby the coincidence and observer interact is light energy and the equations of this branch of Physics ought somehow to formulate the active part which this agency plays. In practice, the quantitative aspect of this agency is introduced by way of experiments on the velocity of light. This light-velocity concept is to play a fundamental rôle in shaping our quantitative and qualitative experience in the spacetime which it characterizes. Thus, we may entertain the thought, that the properties of spacetime which we find experimentally by way of visual coincidences are found by using light beams as probes,[4] using the term probes in the sense of connecting rods between our local scales and clocks, and distant scale and proto-scale distinguishing marks.

These proto-scale distinguishing marks, thought of as wandering amorphously through spacetime, enter the realm of Physics when an observer gives *order* (and therefore, structure) to their spacetime histories, by recording their several coincidences on ordered scale marks. The images of these coincidences are the termini of light beams stretching from the coincidence to the observer, or from coincidence to coincidence and then eventually to the observer. In this complex of coincidences, the observer is to an extent a piece of apparatus; to the extent, in fact, that in most experimentation today he is the ultimate resting place of the coincidence image. As a piece of apparatus, he frequently functions among other ways, to convert the observed image

[4] The term "probe" tends to obscure concepts such as wave-length, wave propagation, etc. Irrespective of the value of or satisfaction from such mediumistic concepts, the strict method of coincidence can, on last analysis, assert no more than experimental relationships between coincidence numbers. It remains to be seen whether emphasis on the coincidence tends to minimize or accentuate the rôle of mediumistic concepts, for the Physicist working in the laboratory.

to a permanent record.[5] (This is done, in cases, automatically by mechanical devices.)

Emphasizing, then, these recorded coincidences we are led to inquire whether the usual difficulties in the interpretation of light-velocity experiments are encountered even though the coincidence rather than the velocity concept is stressed. We shall not attempt a full discussion of this most interesting question here, but merely indicate the direction in which the usual moot questions of interpretation may have an answer. To this purpose we enumerate some of the troublesome topics which seem inevitably to arise when light-velocity experiments are discussed:

(*a*) In view of the fact that in the usual light-velocity experiments, there are no distinguishing marks *within* the light beam stretching from the clock [6] (octagonal mirror) to the distant mirror, can we assert that light has velocity in the same sense that a mass point (a bona fide distinguishing mark) has velocity?

(*b*) If two clocks [13] (initially superimposed and identical) are separated and used to determine the velocity of light (assuming this can be done), can we assume invariance of kinematic clock properties under transport?

(*c*) How can two distant clocks be experimentally declared "in phase"; or what comes to the same thing, can we *define* an "in phase" condition which may be experimentally realized?

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[5] To push on with the picture, we may say that the mathematical physicist takes these numbers, and with pencil and paper, makes conventional marks, obeying definite rules. The end product of the mathematical physicist is again a number, or set of numbers, of such potency, as to move the observer to place his eye in certain places of the spacetime manifold with the expectation of recording an actual coincidence whose order number is that given to him by the mathematical physicist. The rôle of the observer and the mathematical physicist insofar as they dove-tail into a given piece of apparatus with its interconnected scales and indicators (proto-scales) will no doubt be given greater definition with the advances of psycho-physics, or as it is sometimes called, psycho-metrics.

[6] Between two termini of a light beam, within which we have no distinguishing marks, the geometry of a pure continuum appears to have its most ready application. The metrical properties of such a region are formally codified by the Riemannian metric, $ds^2 = g_{ij}dx^i dx^j$; in this continuum the variational principle, $\delta \int ds = 0$, gives us the spacetime accounting of a singularity, thought of as a mass point, while $ds = 0$ gives us the properties of matter-free space, as probed by light beams.

(*d*) Are the scale elements of a measuring rod invariant under transport of the *observer* (a problem which involves the "simultaneity" of two distinct coincidence readings on a given scale by a single observer)? A little reflection will show that this last question is tied up with (*b*) and (*c*) and problems of gauge variance.

Many other pertinent questions may justly be raised. (*a*) and (*c*) which have to do with the synchronization of clocks are particularly troublesome, once we admit that light is propagated through space with a finite velocity. Now, the method of coincidences does not pretend to resolve unequivocally the usual difficulties of interpretation. Its course is quite in another direction, for, by concentrating on the coincidence it has a tendency to side-step the highly conceptual aspects of interpretation of the light-velocity experiments, rather than take issue with them. It will be seen that it renders some of the questions raised somewhat impertinent.[6]

Questions (*b*) and (*c*), in particular, appear to receive satisfactory treatment. The reason for this (as the sequel attempts to show) is that the method of coincidences emphasizes the rôle of the clock as a *dynamical configuration*,[7] rather than a dynamical "catalytic agent" which, itself untouched, is removed after the experiment, leaving us with a highly conceptual variable, t, useful in differential equations. By a study of the methods for the determination of the velocity of light we are further driven to regard the clock as an integral part of the dynamical picture rather than a purely conceptual reference mechanism. It will be seen that the clock emerges as a dynamical structure which requires for itself as much dynamical consideration as, for example, a mass-point whose motion it is helping to plot.

Thus, if clock motion is to be retained as part of the dynamical whole, we may assert that, if an observer use the *arbitrary* motion [7] of a mass-point (a proto-scale mark, giving

[7] The description of any kind of motion from the point of view of an arbitrarily moving mass point, or more correctly, from the point of view of a system of scales and clocks fixed on a mass-point moving in arbitrary relative motion to the mass point under observation, is a question for General Relativity. We desire to emphasize above that the clock hand tip is a mass point performing a specialized kind of motion.

coincidences on his local scales) *as a (pseudo)-clock*, he can describe the dynamics of a usually accepted clock mechanism,[1] in terms of the space intervals on the scale of the distant clock under observation, and the arbitrary time intervals of his own mass-point (pseudo)-clock. One may, of course, raise the following question: Of all the various kinds of dynamical systems we find in spacetime, why should we single out for special classification [1] clock indicator motion?

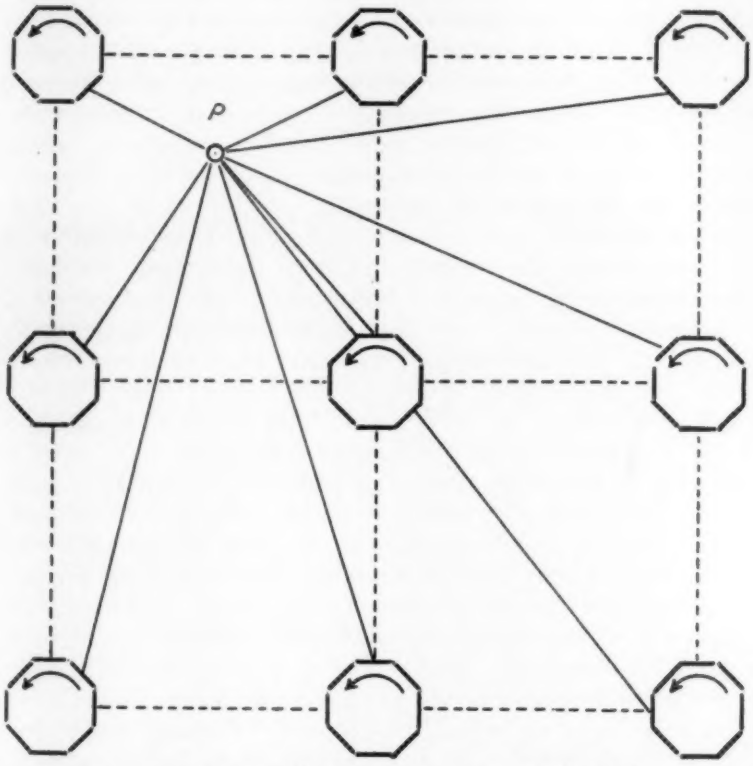
Since this is a question for the psycho-physicist, and not for the physicist (as such), we are compelled to admit that a clock mechanism may be studied with as great impartiality as the arbitrary motion of a mass-point. If, of course, certain mechanisms give us special kinds of motion, and we wish to give this dynamic specialty a name, such as *clock*, or an "entity having spin" [8] we advance our science from the point of view of classification. These singular mechanisms called clocks, however, do offer one difficulty as judged by pre-relativity formulation of dynamical relations. Suppose we wish to record the spacetime coincidences of a distant clock indicator. We use for this purpose a local scale and another (local) clock. This latter reference clock, being itself a dynamic mechanism requires motion analysis. We are therefore compelled to use a third clock, etc., the problem therefore lacks definition. The advent of relativity theory, by providing transformation equations between the recorded data of two observers each viewing the other's clock has encompassed the problem in a form which is satisfactory if we are willing to allow (so to speak) the validity of points of view other than our own, provided they conform to the demands of the equations of transformation.

Employing this relativistic procedure we establish an *array* of specialized dynamical mechanisms, called clocks, and connecting their indicators by light strips, we may give an empirical prescription for synchronization in terms of which each observer is eventually able to make a formal comparison of his own data with those of others. Figure 1, which is anticipatory of the procedure unfolded later, shows a mass point wandering through such an array of clock structures, connected to each of the clocks by a ray of light.

[8] Or from the point of view of microscopics, "a vibrating atom" or a "spinning electron."

The task at hand, then, is to work up to an *empirical* criterion for synchronization of clocks. This empirical synchronization, as will be seen, depending on a macroscopic physical *effect* (the light-velocity effect) appears to lie at the very foundations of an absolute kinematical [1] quantum.

FIG. 1.



A mass-point, P, wandering amongst an array of synchronized clocks.

PART I. THE CLOCK MECHANISM AND THE LIGHT-VELOCITY EFFECT.

Types of Experiments on the Light-Velocity Effect. From a kinematical point of view we may classify the types of light-velocity experiments into:

A. Single Clock Experiments
B. Multiple Clock Experiments.

In single clock experiments a clock is taken to be in synchronism with itself when a certain physical effect, usually a steady, sharp, "return" image is realized.[9] On the other hand, in proposed multiple clock experiments some effect is desirable which may be used as a criterion for synchronization. We shall extend the notion of the single clock, sharp, return image to a multiple array of clocks, and consider the consequences from the point of view of the method of coincidences.

Single Clock Experiments. A typical single clock experiment [9] is that performed by Michelson, with rotating mirrors, whose plane facets were arranged so as to give, in plan view, eight, twelve and sixteen sided regular polygons. These rotating mirrors are to be thought of as clocks. Figure 2 shows the Michelson arrangements [9] with certain modifications in drawing, to emphasize the coincidence features of the experiment. A concentrated point light-image (or sharp line) taking origin on scale mark number zero, is observed as a concentrated point (or line) light-image on scale mark number 4. The light beam (euclidean) space path (euclidean, except for the small portion of the optical path through lenses and prism, etc.) is $SOM_1M_2M_3M_4M_5M_4M_3M_6M_74O = 2\xi$, in which ξ is an amorphous coördinate. By using a scale XX we find from observing coincidences that $2\xi = n_1B_1 + \epsilon_1$, where B_1 is the relative [1] scale quantum of the scale XX. ($nB + \epsilon$, shown in Fig. 2, is one-half, roughly, the light-path.) From the fact that we are employing a clock and a scale, it is natural to associate a velocity with the above experiment. The questions which arise, therefore, are what is the time interval involved, how is it to be measured, and how related to the interval on the scale XX? Now, if we regard the light as being "propagated" through space and time we think of the light as going and returning in the time necessary for one facet to move into the position of the next,

[9] A. A. Michelson, *Astrophysical Journ.*, **60**, 256 (1924). *Ibid.*, **65**, 1 (1927). The revolving mirror clocks are compared with a tuning-fork, which, in turn, is compared with a free seconds pendulum. This pendulum is calibrated against an invar gravity pendulum, which is eventually compared with the earth-clock. The scale of the earth-clock is the *ordered* array of fixed star images, allocated on a system of local cross-hairs of terrestrially coördinated telescopes. The cross-hairs are the proto-scale distinguishing marks (indices) of the earth-clock. The fixed star images are the earth-clock scale marks.

or for the mid-line (or mid-point) of any one facet to move from $n_4 \rightarrow n_4 + 1$, n_4 being the generic symbol for the ordinal number on the clock scale.

We think, also, that if the angular velocity of the clock were doubled, then the relative quantum of the clock (B_4 seconds per scale mark interval of clock scale) would be halved, while any facet index would move from $n_4 \rightarrow n_4 + 2$ during the light's journey from, and back to, the clock. B_4 being determined by local calibration,[9] we have two quantities from the method of coincidences:

$$\Delta t = n_4 B_4 + \epsilon_4 \tag{1}$$

$$\Delta s = 2\xi = n_1 B_1 + \epsilon_1. \tag{2}$$

Let us call the quotient of (2) by (1), c. Thus:

$$c = \frac{\Delta s}{\Delta t} = \frac{2\xi}{\Delta t} = \frac{n_1 B_1 + \epsilon_1}{n_4 B_4 + \epsilon_4}. \tag{3a}$$

In Fig. 2, $n_4 = 4$, i.e. the return image rests on clock-scale mark number 4. Experimentally we may, by proper optical devices, bring the "return" [10] image to any one of the clock-scale marks. Thus for the same "return image effect" or, as we shall hereinafter call it, the "light-velocity effect," n_4 may be 0, 1, 2, $\cdots$ 7, indiscriminately for, say, an octagonal mirror clock.

Let us momentarily, for the sake of clearness, set $\epsilon_1 = 0$ and $\epsilon_4 = 0$. The setting of $\epsilon_4 = 0$ corresponds to the desiderative experimental condition, namely, that the return image shall be bisected by the cross-hair of the observation telescope. In the Michelson experiment,[9] $\epsilon_4 \fallingdotseq 0.002 B_4$, i.e. 0.2 per cent. of the relative clock quantum. Further, the negligibility of ϵ_1 compared with $n_1 B_1$ can be fully realized from the statement by Bowie: [9] "The probable error of the straight-line distance from Mount Wilson to San Antonio peak is 1 part in 6,800,000 from field measurements and

[10] We use the word "return" as a concession to the point of view that light is *propagated*. From the point of view of the symmetrical pattern of light beams and their several scale coincidences shown in Fig. 2, the concept of light propagation seems somewhat impertinent, though, perhaps useful as a bridge between habitual physical picturization and the contemplation of direct sensory effects (such as the "return" image) and their coincidence numbers.

observations alone . . .," so that if $B_1 = 1$ cm. per scale mark interval, $\frac{1}{2}(n_1) \doteq 3{,}542{,}450$ and $\frac{1}{2}(\epsilon_1) \doteq \frac{1}{2}$ cm. Thus we write (3) as [11]

$$c = \frac{n_1}{n_4}\frac{B_1}{B_4}. \tag{3b}$$

In view of (3*b*) what is the velocity of light? The method of coincidences, without any assumptions as to the constancy of c, for the octagonal mirror, gives eight different values, one of which (for $n_4 = 0$) is infinite. We shall discuss this point at some future time; suffice it to say here that for the return image the value $n_4 = 0$ is to be excluded.

If, on the other hand, we *assume* that c is a constant in (3*b*) we have for the light-velocity effect:

$$\left.\begin{aligned} c_1 &= \frac{(n_1)_1}{1}\frac{B_1}{B_4} \\ c_2 &= \frac{(n_1)_2}{2}\frac{B_1}{B_4} \\ &\cdot \\ &\cdot \\ &\cdot \\ c_7 &= \frac{(n_1)_7}{7}\frac{B_1}{B_4} \end{aligned}\right\} \tag{4}$$

in which $c_1 = c_2 = \cdots = c_7 = c$, and $(n_1)_1$ is the value of n_1 for $n_4 = 1$; $(n_1)_2$ is the value of n_1 for $n_4 = 2$; etc.

From (4), we have, then, that

$$(n_1)_{n_4}B_1 = 2\xi = n_4(B_4c). \tag{5}$$

Reinstating our ϵ's, and dropping n_4, as a subscript, we have,

$$\begin{aligned} 2\xi = n_1B_1 + \epsilon_1 &= n_4B_4c + \epsilon_4c \\ &= n_4\left(B_4c + \frac{\epsilon_4c}{n_4}\right), \end{aligned} \tag{6}$$

[11] It is interesting to note that if the euclidean space interval (gauge) between two scale marks of the mirror-clock scale is the same as that of the scale XX (Fig. 2) the $B_1/B_4 = v_4$, the peripheral velocity of the mirror facet midpoint. Hence $v_4/c \doteq n_1/n_4$, using (3*b*), so that if the peripheral velocity of the midpoint of the clock were equal to c a conceptual light particle and the midpoint of the mirror would move over equal paths in the same time interval; a minimum light-path for a given clock participating in the light-velocity effect.

so that the distance mirror M_4 will give the light velocity effect only for certain positions along the line XX'. Thus *relative* to the clock-scale quantum B_4 and the length scale quantum B_1 we may write

$$2\xi = n_4\left[B_4 c\left(1 + \frac{\theta_4}{n_4}\right)\right], \qquad 0 < \theta_4 < 1, \tag{7}$$

since $\epsilon_4 = \theta_4 B_4$.

In the next section we shall show that all observers using clocks synchronized according to a definite empirical prescription will find (7) to be valid, and that consequently 2ξ which represents twice the light path (i.e., the circuital light path) will be *absolutely* quantized, with due respect to the experimental limitations on account of which ϵ_4 differs from zero, in general.

We now return to (3*b*), which, without the assumption of the constancy of c, gives us seven possible finite values for the light-velocity, for a given n_1. This arbitrariness arises out of the fact that we can bring the return image to rest anywhere on the clock scale (with, however, a loss of [12] symmetry in the light-beam pattern in Fig. 2).

This ambiguity is carried over into (7), thought of as an equation to determine 2ξ, for if we are still in the purely empirical stage, in the way to establishing (7) we may (e.g.) double the value 2ξ by placing the distant mirror twice as far away as measured along the scale XX. The right hand side of (7) may, however, empirically take on seven different values. As a preliminary to obviating this ambiguity we proceed as follows: Place the distant mirror M_4 exceedingly far away, but at some value satisfying (7). Now, B_4 will be large, i.e. to say the octagonal mirror will rotate with a small angular velocity. The observer looking through the telescope will not see a steady sharp image, but one varying in intensity,

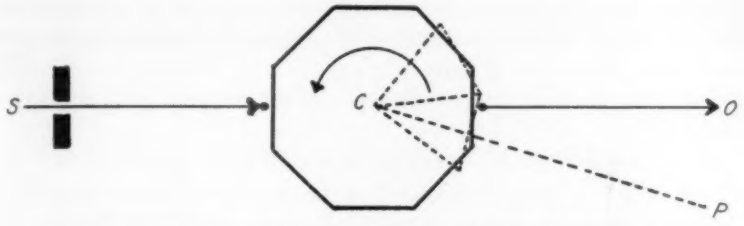
[12] If we demand symmetry, with respect to XX, of the light beam pattern the position on the scale of the return image is uniquely determined. Michelson remarks: [9] "The arrangement of apparatus at the home station was slightly different . . ., the advantage being an increase in intensity in consequence of greater effective width of the light beam falling on the revolving mirror at nearly normal incidence, *as well as greater symmetry*" (the italics are our own). We shall not pursue this point further, in this paper, for we are interested in a multiple clock invention discussed below.

FIG. 2.

The Michelson light-velocity experiment schematically represented so as to throw emphasis on ordinal scale numbers, particularly of the clock scale involved.

the observed intensity of light being a function of the angle between the line of sight *CO* (Fig. 3) and the instantaneous normal to the plane of the mirror facet, *CP*. This observed light will come to a maximum sharpness and intensity periodically, and the effect sought after will be to have the maximum intensity and greatest sharpness occur when the midpoint of the mirror lies on the line *CO*. If we now remove the numbers from the clock scale and attach them to the midpoints of the mirror facets, leaving at scale mark 4 an index, we have a clock whose scale revolves and whose index is stationary with respect to the observer. The observer can now count the number of scale marks between successive bright images and call the difference Δn_4. When 2ξ is decreased B_4 is accordingly increased, the light begins to

FIG. 3.



Intensity of return image is a function of the angle between *CO* and *CP*.

flicker until in the experiments actually performed, B_4 is so small that the "return" image appears sharp and steady. When the image is sharp and steady the counting is done stroboscopically. It may be noted that the stroboscopic [9] image used to calibrate the mirror clock differs from the "return" image, in that in the former the light path is not circuital while in the latter it is. This distinction is important, particularly for an empirical prescription of clock synchronization. The evaluation of $\Delta n_4 = n_4'' - n_4'$ from maximum intensity to maximum intensity cannot be measured directly from viewing the image when B_4 is less than about one-sixteenth of a second per scale mark interval. Hence Δn_4 is read from coincidences of auxiliary scales, stroboscopically. Thus for the light-velocity effect $\Delta n_4 = 1, 2, 3,$

or etc. We therefore write (7) as

$$2\xi_0 = \Delta n_4\left[B_4 c\left(1 + \frac{\theta_4}{\Delta n_4}\right)\right], \tag{8}$$

in which c having been determined by a single experiment (of many trials) implied by (3a) is a number used in (8) which presumes to predict values $2\xi_0$ as an integral multiple of $B_4 c(1 + \theta_4/\Delta n_4)$, the lack of complete relative quantitization being expressed by some statistical average value of ϵ_4, such as its most probable value. In (8) we have replaced ξ by ξ_0, anticipating its absolute quantum aspect which appears from proposed multiple clock experiments, which we now discuss.

Multiple Clock Experiments. In (8) $B_4 c(1 + \theta_4/\Delta n_4) = 2\xi_0$ is in the nature of a kinematical quantum relative to a particular set of clocks and scales. We shall now describe several multiple clock experiments, from which it will appear that, for clocks, relatively at rest, $2\xi_0$ is in the nature of an *absolute* kinematical quantum, i.e. to say, the same $2\xi_0$ will emerge for all observers, whose clocks are empirically synchronized.

Now, actually, the determination of the light-velocity by two or more rotating clocks has been highly unsuccessful.[13] Nevertheless, we shall speak of light-velocity effects of multiple rotating mirror-clocks systems as though the experiments had been performed and leave to future experimentation the verification of our anticipatory description.

If in Fig. 2 we replace the stationary mirror M_5 by an octagonal mirror clock and also insert the half-silvered surface M_S, along with other optical devices suitable for viewing by an observer O_{II}, we have the arrangement of Fig. 4. We might duplicate these arrangements at clocks *III*, *IV*, etc.

[13] Unsuccessful as far as the writer is able to determine. In 1902 ("The Velocity of Light," Decennial Publications, Univ. of Chicago Press) Michelson wrote: "It may (also) be worth mentioning that some preliminary experiments, made about two years ago, have shown that it is not entirely impossible to employ a mechanical method of keeping the two revolving mirrors in a constant phase relation. For instance, it was shown that the vibrations of a tuning-fork could be transmitted over a mile of piano wire with a diminution of amplitude of less than one-half." The shortcomings of this method for clock synchronization with the view toward measuring the velocity of light was soon pointed out by Michelson, himself, among others.

First, the octagonal mirror clock *II* is kept fixed, i.e. non-rotating, and by properly rotating clock *I*, the light velocity effect is established as viewed by O_I or O_{II}. Now clock *II* is rotated, clock *I* rotating, the while, until again, both observers see the steady sharp image. Under these conditions most of the usual questions of simultaneity of readings of the images on the two clocks do not enter, because the images are steady in spacetime. The observers may leisurely communicate with each other as to the steadiness of the respective observed images.[14]

Other clocks may now be added to the array. Each clock is thought of as having an observer, and the image at any clock is thought of as being at the conjugate focus of the image of neighboring clocks. If $2\xi_{I,\,II}$ is the interclock path between O_I and O_{II} as measured by O_I on a scale laid between O_I and O_{II} (assuming the invariance of kinematic scale properties due to transport of observer) and $2\xi_{II,\,I}$ is the distance measured by O_{II}, etc., we may write, in view of (8):

$$\begin{cases} 2\xi_{I,\,II} = (\Delta n_4)_I \left\{ (B_4)_I c \left[1 + \left(\frac{\theta_4}{\Delta n_4} \right)_I \right] \right\} \\ 2\xi_{II,\,I} = (\Delta n_4)_{II} \left\{ (B_4)_{II} c \left[1 + \left(\frac{\theta_4}{\Delta n_4} \right)_{II} \right] \right\} \end{cases} \tag{9a}$$

and

$$\begin{cases} 2\xi_{II,\,III} = (\Delta n_4)_{II} \left\{ (B_4)_{II} c \left[1 + \left(\frac{\theta_4}{\Delta n_4} \right)_{II} \right] \right\} \\ 2\xi_{III,\,II} = (\Delta n_4)_{III} \left\{ (B_4)_{III} c \left[1 + \left(\frac{\theta_4}{\Delta n_4} \right)_{III} \right] \right\} \end{cases} \tag{9b}$$

[14] This intercommunication might take place as follows: O_I telephones to O_{II}: "My image has been steady for some time and still is." O_{II} replies: "My images has been steady some time before you called, while you were calling and still is." O_I: "Received communication, my image has not changed." O_{II}: "My image has not changed since I received your first communication." These messages may go up and back until both observers are assured of the continuousness of their respective images within any two successive communications. As judged by a single observer, this kind of simultaneity has implications quite different from the usual questions of simultaneity.

etc., for pairs of interacting clocks. In the notation of (12) (see next part) we may write quite generally,

$$2\xi_{N,\,(N+M)} = (\Delta n_4)_N \left\{ (B_4)_N c \left[1 + \left(\frac{\theta_4}{\Delta n_4} \right)_N \right] \right\},$$

which is symmetrical in N and $(N + M)$.

PART II. FORMAL SYNCHRONIZATION AND THE ABSOLUTE KINEMATICAL QUANTUM FOR CLOCKS RELATIVELY AT REST.

We now seek for a formal prescription of synchronization of clocks relatively at rest. The "at rest" condition is imposed in order to establish the physical reality of a galilean spacetime scale (treated in the next section) embedded in the amorphous 4-way coördinate system of relativity theory. What is more natural than to agree that *when the light-velocity effect obtains* the several local B_4's of the clocks of our lattice are numerically equal, i.e. any particular observers' B_4 is numerically equal to that of any other observer? It is, in fact, somewhat difficult to see how any assertion at variance with the numerical equality of the B_4's is compatible with the implications of the term "synchronization." Thus for a necessary condition of rest synchronization we write

$$(B_4)_I = (B_4)_{II} = (B_4)_{III} = \cdots = (B_4)_N \qquad (10)$$

for a lattice array of clocks giving the light-velocity effect.[15]

The next step in formal synchronization is the recognition of the fact that a reading $(n_4)_I$ (in contradistinction to a difference $(\Delta n_4)_I$) which should correspond to a reading $(n_4)_{II}$, or $(n_4)_{III}$, etc., is an entirely arbitrary matter, as has been pointed out. In fact, this correspondence appears so arbitrary that we are at liberty to introduce a convention, compatible, of course, with the restriction that the Δn_4's in (9) shall be an integer. Now, in view of (10), we may interpret equation (9) thus: In an array of light-connected

[15] For clocks in uniform relative motion showing the (presumably persisting) light-velocity effect, the Lorentz proper time intervals are set equal to each other for synchronization of two or more spacetime scales. Two clocks in relative uniform motion are used as a basis for establishing *two* galilean spacetime scales. We are concerned here with the question of establishing a single type of coördinate system in which all the clocks are relatively at rest and not with the relative motion of two such coördinate systems.

clocks, giving the light-velocity effect, the interclock optical paths, $2\xi_{I,\,II} = 2\xi_{II,\,I}$, or $2\xi_{II,\,III} = 2\xi_{III,\,II}$; etc. are integral multiples of the least of the $\xi_{N,\,N+M}$'s, with due respect to the empirical quantum defect $(\theta_4)_N$, which, last, in the idealization of the light-velocity effect, is zero.

We thus have a giant lattice of synchronized clocks, at each lattice point of which, there is a sharp steady concentration of light image in a point or line—concentrated in the ordinary 3-way (say, e.g. euclidean) space. Our convention is this: That when clock I reads n_4, then clock *II* reads

$$n_4 + 1, \quad n_4 + 2, \quad n_4 + 3, \quad \text{or} \quad \text{etc.},$$

according as

$$\xi_{N,\,(N+M)} = 1(2\xi_0),\ 2(2\xi_0),\ 3(2\xi_0), \text{ or etc.},$$

where $2\xi_0$ is the minimum interclock light-path (circuital). This we take as another necessary condition for synchronization.

(10) and (11) together with the light-velocity effect are sufficient to prescribe for synchronization.

Recapitulating then we have for our criterion for synchronization:

$$\left.\begin{array}{l} (a)\ \text{The light-velocity effect,} \\ (b)\ (B_4)_I = (B_4)_{II} = \cdots = (B_4)_N = \cdots = (B_4)_{N+M} \\ (c)\ (n_4)_{N+M} = (n_4)_N \pm M, \text{ in phase condition,} \end{array}\right\} \quad (12)$$

where N is the ordinal number of any clock of the array and M is the ordinal number of another clock referred to N. It is hardly necessary to point out the intrinsic integral property of N and M, arising out of the fact that they are *ordinal* numbers.

(12) is intended to supplement the criterion for synchronization in amorphous spacetime, which asserts that if an electromagnetic signal is dispatched from any point P_1 to any other point P_2 at which it is reflected back to P_1, and if the times of these events as measured at the points in question are t_1, t_2 and t_3 respectively, then P_1 and P_2 are synchronized if

$$t_2 - t_1 = t_3 - t_2.$$

Thus *any* two points in an amorphous coördinate system may be synchronized if we ignore the question of the clock as a dynamical mechanism, whereas (12) selects a discrete set of points from such a system. We may think of amorphous synchronization as applying particularly *between* the clock-points determined by (12).

Since each, and, therefore, every observer will (if the multiple light-velocity effect exists) determine, by measurement, the same $(2\xi_0)_N = (B_4)_N c$, $2\xi_0$ is in the nature of an *absolute* kinematical quantum. This absolute kinematical quantum as determined by Michelson [6] is:

$$2D = 2\xi_0 = 2(35424.5) \text{ meters, kinematical,}$$

using a rotating octagonal mirror clock of

$$B_4 \fallingdotseq 0.000237 \text{ second,}$$

which latter, may be taken as a common value for a multiple array of clocks synchronized in accord with (12*b*).

PART III. THE SPACETIME (ABSOLUTE) SCALE.

The Spacetime Scale as a Laboratory Possibility. Consider the array of synchronized clocks of Fig. 2. The absolute kinematical quantum as determined by Michelson, $2\xi_0$ (the circuital property of the light-path is emphasized by retaining the numerical factor 2), is inconveniently large for a laboratory spacetime scale. It may be made smaller by increasing the angular velocity, i.e. the rotational frequency, of the rotating mirror clocks. This amounts to making $(B_4)_N$ smaller, for a given mirror-clock scale.[16] The project of making $(B_4)_N$ small enough to give the light-velocity quantum effect, all under one laboratory roof, though beset with great

[16] We may *conceive* of $(B_4)_N = 1/3 \times 10^{-18}$ seconds per scale mark interval. Then $2\xi_0 = 10^{-8}$ cm. (kinematical). This size of $2\xi_0$ may be more or less useful depending on circumstance. *In a continuum* its conceptual implications are not different from $2\xi_0 = 2(35424.5)$ meters (kinematical). The writer is well aware that this pronouncement may be justly quarreled with, on many different philosophical highways. To state that conceptual differences between large and small units in a continuum are of degree only and not of kind is to partially define the terms "conceptual implications" and "continuum." Accepting this corrective, we shall adhere to the above pronouncement, admitting that it places a limitation on the concept of continuum.

mechanical difficulties, is nevertheless worthy of contemplation. In this connection the experiments of Henriot and Huguenard [17] may be mentioned.

Small mirror-clocks spinning on cushions of air were given angular speeds of 11,000 rotations per second. The diameters of these clocks are about 1.17 cm. Such a pair of clocks with 50 facets each, would have about 1800 feet of optical path between them for the light-velocity effect, or one-half this between mirror-clocks. If these experiments were pushed harder the light-paths with multiple reflections may turn out manageable from a laboratory point of view. Such experiments would be particularly valuable in making direct observations on the light-velocity as a function of relative motion of mirror clocks.

The Spacetime Scale as a Structure Coördinate System. Figure 1 or 4 may be thought of as a galilean spacetime scale built up of an array of synchronized clocks. To every lattice point corresponds three euclidean coördinates and a time coördinate. The origin of coördinates may be taken at any clock, i.e. to say at any observer's clock. One of the essential distinctions between the spacetime scale and the amorphous coördinate system is this: That while any four coördinates x^1, x^2, x^3, and x^4, determine a spacetime point amorphously, i.e. irrespective of the order of the distinguishing marks from the origin to the point in question, the coördinates of the spacetime scale are automatically *ordered* by the clock reading according to the criterion for synchronization given by (12). Thus the time reading for the structure coördinate gives the number of (clock) steps necessary to get to the clock in question; and conversely, the number of steps necessary to get to a particular clock gives uniquely the clock reading at the point.

In this sense our spacetime scale is a quantum scale, the departure from complete quantitization, as has been pointed out, being a statistical function of the magnitude of ϵ_4.

If now we think of a single observer at one clock (the origin) of a spacetime scale and emphasize the *functional*

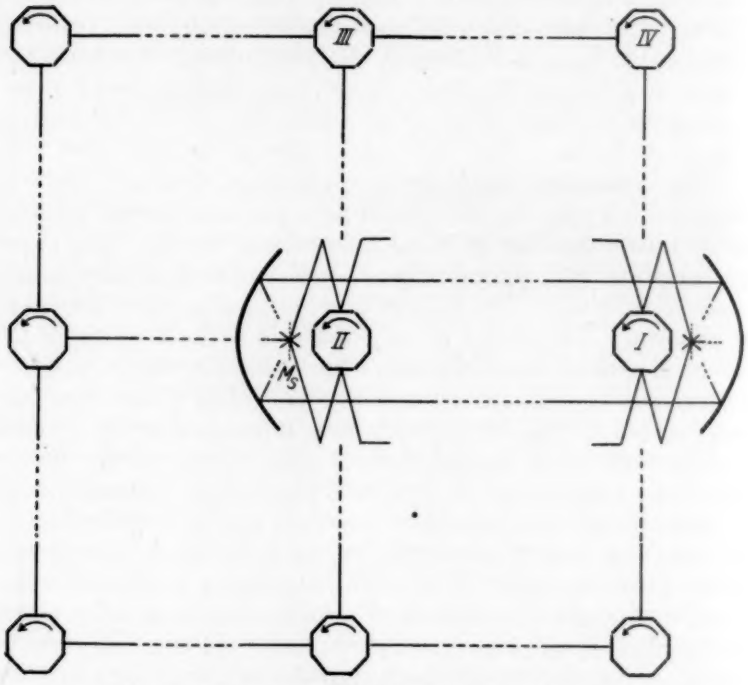
[17] *Journal de Physique et le Radium*, **8**, 433, Nov. (1927), Series 6, No. 11.

quantum relationship:

$$\xi^i = (\Delta n_4)^i \left\{ (B_4 c) \left[1 + \left(\frac{\theta_4}{(\Delta n_4)} \right)^i \right] \right\} \qquad i = 1, 2, 3. \quad (13)$$

We see that on the spacetime scale the number of independent coördinates are reduced from four to three, in contradistinction

FIG. 4.



Schematic diagram of a multiple clock experiment for the purpose of empirical synchronization.

to the four independent coördinates x, y, z and t of an amorphous coördinate system. This reduction is wrought through the replacement of the simultaneity concept associated with the amorphous variable t (i.e. $t_2 - t_1 = t_3 - t_2$) by an empirical criterion for synchronization, (12), formulated in terms of scale elements n, B and ϵ, of both length scales and clock scales.

Those coincidences lying between two clocks have an essential uncertainty (expressible in amorphous space) in the time reading, whose range is given by

$$0 < \epsilon_4 < B_4 \tag{14}$$

and a space (e.g. euclidean) uncertainty, of range,

$$\left.\begin{aligned} 0 < \epsilon_4 c < B_4 c \\ 0 < \epsilon_1 < B_1. \end{aligned}\right\} \tag{15}$$

or,

On a galilean spacetime scale, the interval between two spacetime coincidences is given by:

$$\begin{aligned} (\overline{\Delta s})^2 &= (\xi^1)^2 + (\xi^2)^2 + (\xi^3)^2 \\ &= \sum_{i=1}^{3} \{(\Delta n_4)^i (B_4 c) + \epsilon_4{}^i\}^2. \end{aligned} \tag{16}$$

Introducing the concept of the common [1] scale quantum we write

$$\Delta s = nB + \epsilon$$

and setting

$$(\Delta n_4)^i \equiv n_4{}^i,$$

(16) becomes,

$$(nB + \epsilon)^2 = \sum_{i=1}^{3} \{n_4{}^i (B_4 c) + \epsilon^i\}^2. \tag{17}$$

We now identify B with $B_4 c$ and for pure coincidences on the spacetime scale, i.e. for lattice point coincidences, we have the quantized position vector whose magnitude is given by,

$$nB + \epsilon = \{(B_4 c)^2 [(n_4{}^1)^2 + (n_4{}^2)^2 + (n_4{}^3)^2]\}^{\frac{1}{2}}. \tag{18}$$

On a one-dimensional spacetime lattice the spacetime uncertainty, $\epsilon = 0$, when $n = n_4{}^1$. On a two dimensional spacetime lattice $\epsilon = 0$ when $n^2 = (n_4{}^1)^2 + (n_4{}^2)^2$, while on a three dimensional spacetime lattice, $\epsilon = 0$ when

$$(n)^2 = (n_4{}^1)^2 + (n_4{}^2)^2 + (n_4{}^3)^2.$$

For such coincidences the direction cosines of our position vector are given by

$$\alpha^i = \frac{n^i}{n}, \tag{19}$$

which selects a discretely infinite set of quantized space vectors on a spacetime scale. *Points for which* ϵ *(the space-time uncertainty) vanishes in* (17) *may be said to be synchronized.*

As was pointed out,[1] a fundamental kinematical property of a clock is $v_4 = B_1/B_4$; a fundamental geometrical property is its radius. If there are N scale marks on a clock scale, there are N space intervals on the clock scale. If R is the clock radius, we have

$$B_1 = \frac{2\pi R}{N}$$

for the clock scale, and

$$v_4 = \frac{B_1}{B_4} = \frac{2\pi R}{NB_4},$$

whence

$$B_4 = \frac{2\pi}{N\frac{v_4}{R}}$$

$$= \frac{2\pi}{Ns}$$

$$= \frac{1}{Nf},$$

where s (spin) is the angular velocity of the clock, and f is the number of cycles (space on time) which the index is making per unit time on the clock-scale.

But

$$B = B_4 c$$

$$= \frac{c}{Nf} = \frac{c}{\nu} \equiv \lambda_0,$$

where $\nu \equiv Nf$ is the number of intensity maxima of return image per unit time. (17) becomes

$$(n\lambda_0 + \epsilon)^2 = \sum_{i=1}^{3} \{n_4{}^i(B_4 c) + \epsilon^i\}^2. \qquad (20)$$

In (20) we have the kinematical equation for a galilean spacetime scale or lattice. We may call such a structure a macroscopic *galilean* "crystal" lattice. (20) suggests that

an actual crystal, possessing other than simple galilean kinematical properties may be described by

$$(n\lambda_0 + \epsilon)^2 = e g_{ij}[n_4{}^i(B_4{}^i c) + \epsilon^i][n_4{}^j(B_4{}^j c) + \epsilon^j], \qquad i, j = 1 \cdots 3 \quad (21)$$

where g_{ij} are those macroscopic scaled quantities for the crystal, in which small changes sensibly alter Laue patterns, for example. These patterns consisting of electromagnetic images, i.e. concentrations in euclidean space of electromagnetic energy, form coincidences (e.g. on photographic plates).

The question which arises is this: Shall these images (Laue spots) be referred to an euclidean reference frame or do they form of themselves a non-galilean (or in special cases galilean) spacetime lattice? In other words, shall the natural formation of arrays of concentrated light (electromagnetic, more generally) images in lines or spots,[18] be described in amorphous coördinates, or do they constitute a spacetime light pattern of "synchronized" points which is of itself a coördinate system? If we admit that light image patterns form a quantum coördinate system, then the quantum problem, insofar as it treats of these image formations, appears to arise in the attempt to establish transformation equations between these spacetime scales and a uniform [1] length scale, such as is found on a comparator.

Résumé. It may be well to give a brief résumé of the steps leading to the formulation of an absolute kinematical quantum.

(i) First of all, to assert the numerical equality

$$(B_4)_I = (B_4)_{II} = \cdots = (B_4)_N \qquad (12b)$$

is not to demand necessarily the existence of an absolute kinematical [19] quantum. We are still faced with the problem

[18] We may include here the line images when our space is formed by excited nitrogen, for example, refracting prisms and lenses, and a photographic plate.

[19] The term "kinematical" as used here refers to the *Physics* of relative motions and in this sense implies a measurable physical world. Thus, we may briefly define kinematics as the mathematics of the relative motions of scale and protoscales which somehow formularizes the fact that these coincidences are *connected by light beams.* The introduction of the light beam as a kinematical concept, by way of the velocity of light, has been one of the outstanding contributions of Einstein in kinematical theory.

of communication (between observers), without which (12*b*) is in the nature of a ***mathematical postulate*** closely associated, to be sure, with physical concepts.

The introduction of the element of communication immediately gives rise to two additional considerations:

(ii) The euclidean displacement between clocks, $2\xi_0$.

(iii) The agency (electromagnetic energy) by which communication is effected.

Contemplation of $(B_4)_N$ and $2\xi_0$, together, leads to the concept of velocity, which by inference has been attributed to the agency (light) employed for communication.

Which of the following shall we choose as our fundamental quantum?

$$B_4 = \frac{2\xi_0}{c}, \tag{a}$$

$$c = \frac{2\xi_0}{B_4}, \tag{b}$$

or

$$2\xi_0 = B_4 c. \tag{c}$$

It appears to the writer that the choice made is a matter of what one wishes to emphasize from an experimental point of view. We have chosen $2\xi_0$, in this paper, for it allows us to explore space in terms of a local magnitude B_4 and a previously determined spacetime constant c.

GAMMA RAY RADIOGRAPHIC TESTING.*

BY

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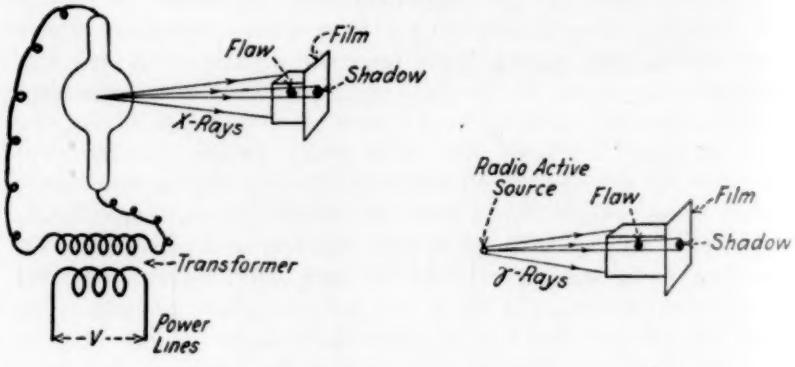
INTRODUCTION.

Electromagnetic waves of several types exhibit the ability to enter and pass through "opaque" matter and to emerge on the opposite side thereof with only partial loss of their radiant intensity. Thus the relatively long electromagnetic waves, known as wireless, or radio waves, pass through the walls and roofs of buildings in program broadcasting and reception, without the necessity of outside antennæ. Waves of intermediate length such as the heat waves, infra-red waves and visible light waves, lying near the middle of the electromagnetic spectrum, do not in general penetrate opaque matter nor do the ultra-violet waves which are found just above the visible range. Above the ultra-violet range, lie the still shorter X-rays, gamma rays and possibly the cosmic rays. With these rays of very short wave-length is recovered again the ability to pass through opaque matter, such as is shown by the long radio waves. Unlike the long radio waves, however, these short waves will affect the light-sensitive photographic film upon which they fall, as does visible light. They present therefore the possibility of exploring the interior of a solid opaque object, such as a decayed tooth or a broken arm by permitting such short rays to pass through the tooth or arm and thence onto a photographic film where they record approximately what degree of absorption they experienced in their passage through the opaque object. The intensity of the rays is, partially diminished by absorption in the object through which they pass, and since the amount of absorption is roughly proportional to the density of the absorber, those regions of low density in the object such as voids, cracks, slag and sand inclusions in a metal object lying in the path of the rays, will

* An address presented in condensed form before the Institute on the evening of 10 November, 1932.

absorb less of the radiant energy than will the surrounding homogeneous material and consequently the rays passing through these low-density regions will be registered with a greater intensity on the film, and produce dark spots on the film in line with the flaws in the object. The entire process of radiography is thus closely analogous to that of contact printing in photography, in which light falls on a negative and passes through it to a light-sensitive printing paper. The light areas on the negative (regions of low density in the object) absorb the light rays less than the dark areas do, and make the print dark where the negative was light (less dense), etc. The X-rays have been used with gratifying success in this way. The simplest possible illustration of this method, called "radiography" is given in Fig. 1*a*. The X-rays ema-

FIG. 1.



a *b*

nate from a metallic target (not shown in sketch), which is bombarded by electrons liberated opposite the target and accelerated toward the target by an electrical potential gradient, all enclosed in a highly evacuated tube. The rays pass through the test object, shown with a flaw at its centre, and register a shadow of the flaw on the photographic film fastened behind the object. The method of exploring the interiors of opaque objects by electromagnetic waves is called "radiography."

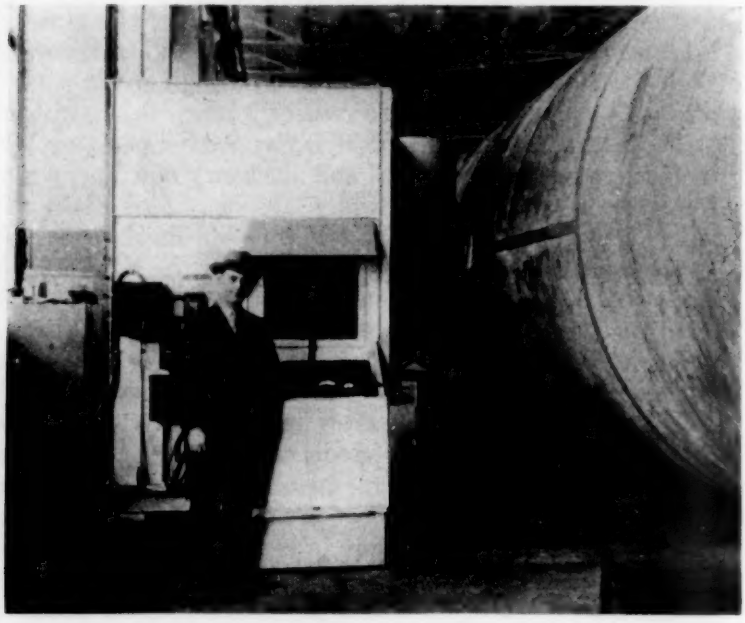
The X-rays when used in this manner have been satisfactory for exploring the interior of the human body, the

interior of cotton bales, of radio tube assemblies (through an opaque exterior), and for examining the soundness of the interior of metal objects of considerable thickness for example steel objects up to 4 inches thick. Where fineness of detail is desired, as in biological examination, the longer (softer) X-rays, lying near the bottom of the X-ray range, are used. These rays are produced by the use of a low voltage in the tube, such as 30,000 volts. Where deep penetration through dense metal objects is necessary, the shorter ("harder") X-rays, near the top of the X-ray range, are used. The generation of these rays requires from 150,000 to 400,000 volts across the terminals of the tube. Unidirectional current at this voltage is obtained from an alternating current source by means of transformers and rectifiers. Since the X-rays in large doses are biologically destructive, the operators are protected by enclosing the tube and accessory machinery in a lead-lined room the lining of which absorbs most of the radiation. Figure 2 shows at the left the lead-lined room and the controls of an X-ray testing plant, and at the right a large pressure vessel, the welded joints of which are being tested. The X-rays pass through the weld and fall upon a suitable film fastened snugly against the inside wall of the vessel. There are to-day nearly one hundred equipments of this type in use in the metal industry alone. The presence of internal voids, slag and sand particles, shrinkage cracks and in some cases segregation in metal objects can be detected without destruction of the object or impairment of its salability and the method is therefore of considerable commercial value. Obviously, the radiographic method is useless, however, in detecting cracks in which the two conjugate surfaces are pressed closely together, leaving no open space to permit more intense penetration by the rays.

It was not dissatisfaction with the radiographic results obtainable with X-rays, nor with the cost of the method, that led to the recently completed experiments with the shorter electromagnetic waves called gamma rays. Primarily, it was the necessity of examining sections of steel thicker than $3\frac{1}{2}$ inches, which was in 1928 the upper practical limit with X-rays. This necessity was a pressing one in industry. Rational processes indicated that waves still shorter than the

hardest X-rays, for instance the gamma rays, should penetrate readily up to 8 or 10 inches of thickness of iron or steel and should register their intensity on a photographic film after their emergence from the object. Gamma rays would have to be tried. Secondly, the bulkiness and complexity of the X-ray equipment required that it remain practically anchored and in connection with a source of electric current, whereas

FIG. 2.



X-radiography of welded vessel.

it was frequently necessary to examine a very large, or stationary, object or structure which could not be transported to an X-ray laboratory. A method employing gamma rays would be a portable one. For a capsule less than one-quarter inch in diameter would contain enough radio-active substance for examining practically any conceivable object (see Fig. 1*b*). It could be transported to different shops and floors of a large manufacturing plant, or carried to a remote region where

electric current is unavailable. It could be used on ships at sea or along transcontinental pipe-lines. No complicated machinery of any kind nor any wiring systems would be required, hence no expert mechanical or electrical technicians or assistants would be necessary. The obsolescence rate would also not be that of complex machinery but would be very low.

The pioneering experiments in the use of gamma rays for radiography were carried out at the Naval Research Laboratory on the Potomac River just below Washington, D. C., in the Summer and Fall of 1929 in collaboration with Dr. Robert F. Mehl, Superintendent of the Division of Physical Metallurgy assisted by Dr. Charles S. Barrett of the same Division. Upon Dr. Mehl's invitation the present author collaborated in the project, being granted special leave of absence from Lehigh University for that purpose. The results were published [1] in September, 1930. These experiments established completely and for the first time the entire practicability or radiography of thin sections of metal by gamma rays, and in addition yielded a practicable method for testing metal sections of thickness beyond the 3 or 4 inches which was then and is now the upper limit possible with X-rays, extending it, in fact, up to 8, 10 and even 12 inches thickness of steel, into the regions where the use of X-rays had not previously been contemplated.

In December of the same year the results of further quantitative work in charting the most efficient exposure times was published.[2] These studies were of great assistance in the quantitative exploration of the method and of its industrial application.

In 1931 the present author continued the quantitative studies of the method at Lehigh University in the testing of both sound and defective welds.[3]

The results of tests carried out on some of the castings of the Navy's new cruisers have also been published.[4]

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[1] Mehl, Doan and Barrett, "Radiography by the Use of Gamma Rays," *Transactions A. S. S. T.*, 1930.

[2] Barrett, Gezelius and Mehl, "The Technique of Radiography by Gamma Rays," *Metals and Alloys*, Dec. 1930.

[3] Gilbert E. Doan, "Inspection of Welds with Gamma Rays," *A. S. S. T.*, 1931.

[4] R. F. Mehl, *Am. Soc. Naval Engrs.* (1931), p. 371.

Early in 1932 two German investigators, R. Berthold and N. Riehl in Berlin published [5] the results of a very thorough and valuable, although similar, investigation into the use of gamma rays, in which they reached similar conclusions as to the practicability of the method. The results of these investigations will be referred to hereinafter as those of the German investigations.

Indirect reports of an investigation in Russia, in which an electrical instead of a photographic method was used for recording the rays after passing through the object, have been received.[6]

Certain phases of the new development are receiving continued study both in the Navy Department and at Lehigh University. Its use in industry is increasing.

The present paper is a critical summary of the science and technology of the new method in its present status.

SOURCES OF GAMMA RAYS; THEIR PROPERTIES AND MEASUREMENT.

The radioactive chemical elements are the chief source of gamma rays.[7] Table I [8] lists the radioactive elements with the rays which each emits, and the principal means of preparation of each, as well as other significant data. The symbols in parenthesis (β) or (γ) are intended to denote that β or γ rays of low penetrating power are given off.

Figure 3 [9] shows the positions of radioactive elements in the periodic table. Accompanying the decomposition of the atomic nuclei illustrated above occurs the phenomenon called radioactivity, namely the spontaneous emission of three types of radiation, called alpha, beta and gamma radiations. (Only those atoms which are breaking down at any given instant are

[5] *Z. Verein deutscher Ingenieure*, Bd. 76 (1932), p. 401.

[6] Science News Service, No. 361E, Sheet 1, Feb. 24, 1928. It has been impossible to obtain information by correspondence in addition to the meager report in this sheet.

[7] E. Rutherford, "Radioactive Substances and their Radiations," Cambridge, 1913. K. W. F. Kohlrausch, "Handbuch der Experimental-physik," Teubner, Berlin, 1927. Meyer u. Schweidler, "Radioaktivität," Teubner, Berlin, 1927. F. Soddy, "Chemistry of Radioactive Elements" (Longmans, Green).

[8] Evans, "Metals and Metallic Compounds," Vol. I, pp. 452–456 (Longmans, Green).

[9] F. Soddy, *Trans. Chem. Soc.*, 115, 1 (1919).

TABLE I.

Name of Element.	Rays.	Period of Half-Change.	Group of Periodic Table.	Chemical Character.	Means of Preparation.
Actinium.....	(β)	20 years	IIIA	Homologue of Lanthanum. Resembles Rare Earths	From Pitchblende. After removal of polonium with hydrogen sulphide, iron and rare earths are precipitated with ammonia, and redissolved in hydrochloric acid. Rare earths (including actinium) are precipitated with oxalic acid and fractionally crystallized as double magnesium nitrates.
Radio-actinium...	α	19.5 days	IVA	Isotope of Thorium	By growth from actinium. Thiosulphate is added to an old acidified solution of actinium salt; the sulphur precipitate carries down radio-actinium.
Actinium X.....	α	11.4 days	IIA	Isotope of Radium	By growth from actinium. By precipitating a solution of actinium preparation with ammonia and filtering. Actinium X remains in filtrate.
Actinium Emanation.....	α	3.9 seconds	O	Isotope of Radium Emanation	By growth from actinium. Liberated on passing air through an actinium preparation.
Actinium A.....	α	.002 second	VIB	Isotope of Polonium	Too short-lived for isolation.
Actinium B.....	(β) (γ)	36.1 minutes	IVB	Isotope of Lead	Constituents of "Active Deposit" formed by actinium emanation on walls of vessels. Separated by heat; Actinium B volatilizes more readily than Actinium C.
Actinium C.....	α	2.15 minutes	VB	Isotope of Bismuth	
Actinium D.....	β, γ	4.77 minutes	IIIB	Isotope of Thallium	Acid solution of Active Deposit is treated with animal charcoal, which adsorbs Actinium D.
Actinium E.....	None	Infinite (??)	IVB (?)	Isotope of Lead (?)	

TABLE I.—*Continued.*

Name of Element.	Rays.	Period of Half-Change.	Group of Periodic Table.	Chemical Character.	Means of Preparation.
Radium C′.....	α	10^{-6} seconds (approx.)	VIB	Isotope of Polonium	Too short-lived for isolation.
Radium D.....	(β) (γ)	16.6 years	IVB	Isotope of Lead	By growth from radium emanation, contained in quartz flask. After some weeks the deposit on flask is dissolved in nitric acid and the radium D is then deposited by electrolysis on a cathode.
Radium E.....	β	5.0 days	VB	Isotope of Bismuth	By growth from radium D. An old solution of a salt of radium D on electrolysis at low current density deposits polonium on cathode, and at higher current density radium E (separation usually incomplete). Polonium can also be obtained from carnotite or pitchblende by precipitation of acidified solution with hydrogen sulphide.
Radium F..... (Polonium)	α	136 days	VIB	Homologue of Tellurium, but resembles Bismuth	
Radium G.....	None	Infinite (??)	IVB	Isotope of Lead	From carnotite or pitchblende; separation as for lead.
		Uranium Family. Actinium Branch.			
Uranium Y.....	β	25 hours	IVA	Isotope of Thorium	When uranium preparations containing iron are precipitated with ammonium carbonate for the separation of uranium X_1, much uranium Y is mixed with the uranium X_1 prepared.[3] Uranium Y cannot be prepared free from uranium X_1.
Eka-Tantalum....	α	1.2×10^4 years	VA	Isotope of Uranium X_2	From pitchblende by distillation in current of air and carbon tetrachloride vapour (traces only obtained, mixed with other bodies).

TABLE I.—*Continued.*
Uranium Family. Commencement and Radium Branch.

Name of Element.	Rays.	Period of Half-Change.	Group of Periodic Table.	Chemical Character.	Means of Preparation.
Uranium I.....	α	5.5×10^9 years	VIA	Homologue of Tungsten	From ores like Carnotite or Pitchblende. Methods described in Vol. II.
Uranium X_1.....	(β)	24.6 days	IVA	Isotope of Thorium	By growth from Uranium I. An *old* solution of uranium salt containing a trace of iron is treated with ammonium carbonate, which precipitates uranium X_1 along with the iron.[1]
Uranium X_2.....	β, γ	1.14 min.	VA	Homologue of Tantalum	By growth from uranium X_1. A solution of UX_1 salt is filtered through moist tantalic acid, which extracts UX_2.
Uranium II.....		2×10^6 years (?)	VIA	Isotope of Uranium I	Preparations of uranium X_1 gradually lose their β-ray activity. When the β-ray activity has disappeared, the preparation contains uranium II.
Ionium.....	α	7×10^4 years	IVA	Isotope of Thorium	From solution of Pitchblende in nitric acid. Lead, radium, etc., are precipitated as sulphates, and then ionium, thorium, etc., are precipitated as fluorides. Being isotopic they are inseparable from one another.
Radium.....	α	1,645 years	IIA	Homologue of Barium	From Carnotite or Pitchblende. Methods described in Vol. II.
Radium Emanation (Niton)....	α	3.85 days	O	Inert Gas. Homologue of Xenon	By growth from radium salt solution. Evolved on boiling or bubbling air through the solution.
Radium A.....	α	3.0 minutes	VIB	Isotope of Polonium	Main constituent of "active deposit" at moment of deposition.
Radium B.....	(β) (γ)	26.7 minutes	IVB	Isotope of Lead	From "active deposit" 20 minutes after deposition. Separated from each other by heating at 600°, which volatilizes radium B, and leaves radium C.
Radium C.....	β	19.5 minutes	VB	Isotope of Bismuth	
Radium C_2.....		1.32 minutes	IIIB		Formed in too small amount for isolation.

TABLE I.—*Continued.*

Name of Element.	Rays.	Period of Half-Change.	Group of Periodic Table.	Chemical Character.	Means of Preparation.
			Thorium Family.		
Thorium.....	α	1.7×10^{10} years	IVA	Homologue of Zirconium	From ores such as monazite or thorite. See Vol. II. Extracted on large scale for Mantle Industry.
Mesothorium 1....	(β)	6.7 years	IIA	Isotope of Radium	By-product of Mantle Industry. See Vol. II.
Mesothorium 2....	β (γ)	6.2 hours	IIIA	Isotope of Actinium	By growth from mesothorium 1; separated from it by precipitation with ammonia.
Radio-thorium....	α	696 days	IVA	Isotope of Thorium	By growth from mesothorium. A trace of zirconium is added to the solution and ammonia added. The precipitate of zirconium hydroxide carries down the radio-thorium.
Thorium X.....	α	3.7 days	IIA	Isotope of Radium	By growth from thorium. Old thorium solution is treated with ammonia which precipitates thorium and radiothorium, and leaves thorium X in filtrate.
Thorium Emanation.....	α	54 seconds	O	Isotope of Radium Emanation	By blowing air over a thorium preparation which has been stored long enough to develop thorium X.
Thorium A.....	α	0.14 second	VIB		Too short-lived for isolation.
Thorium B.....	(β)	10.6 hours	IVB		Constituents of Active Deposit. Separated by distilling away the more volatile thorium B.
Thorium C.....	α, β	60 minutes	VB		
Thorium C'.....	α	10^{-11} seconds	VIB	Isotope of Polonium	Too short-lived for isolation.
Thorium D.....	β, γ	3.1 minutes	IIIB	Isotope of Thallium	Separated from solution of active deposit by addition of potassium salt, followed by platinichloric acid. The precipitate of potassium platinichloride carries down the thorium D.
Thorium E.....	None	Infinite (??)	IVB	Isotope of Lead (?)	

[1] For alternative methods, see G. Kirsch, *Sitzungsber. Akad. Wiss. Wien.* 129 (1920) [2a], 309.
[2] See G. N. Antoneff, *Phil. Mag.* 22 (1911), 419; *Le Radium* 10 (1913), 406.

[1] For alternative methods, see G. Kirsch, *Sitzungsber. Akad. Wiss. Wien.* 129 (1920) [2a], 309.
[2] See G. N. Antoneff, *Phil. Mag.* 22 (1911), 419; *Le Radium* 10 (1913), 406.

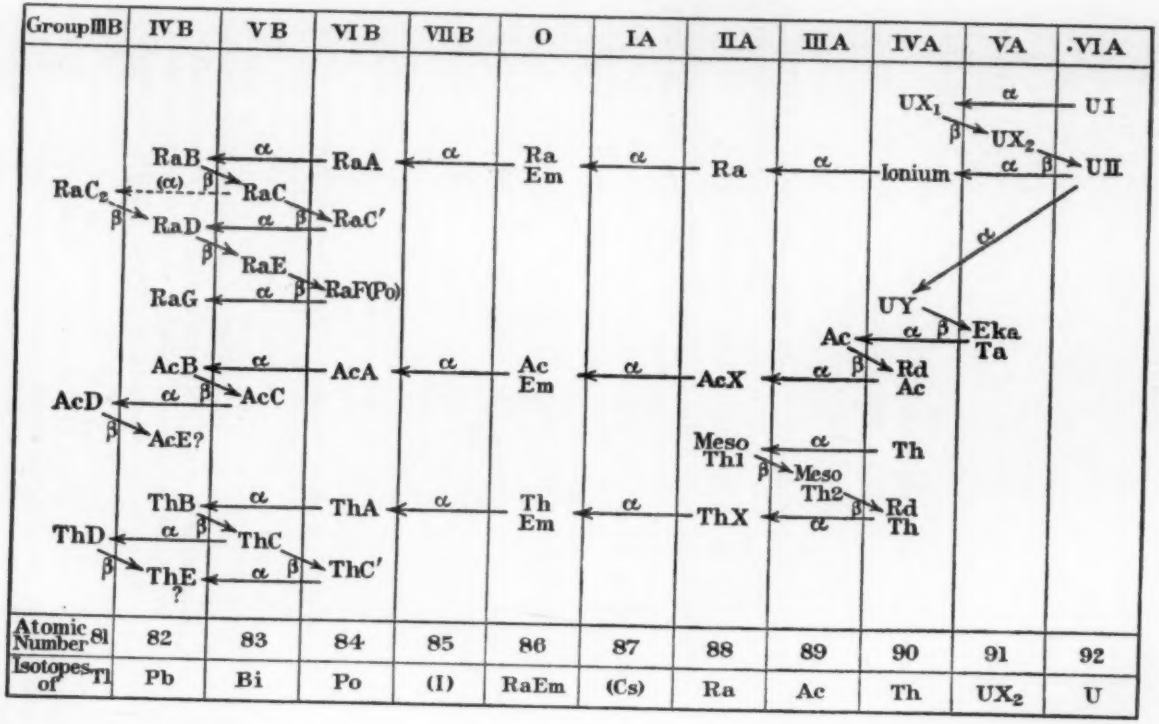


FIG. 3.

responsible for the radiations.) The alpha radiation consists of moving helium nuclei bereft of two orbital electrons and therefore with a double positive charge. These particles are readily stopped by a sheet of paper. The beta radiation consists of moving negative charges of electricity called electrons. These rays are for the most part stopped by 1 mm. of lead. The gamma radiation is one of electromagnetic waves possessing great penetrating power. It should be noted here that the radiations are emitted from the source with equal intensity in all space directions; they are "broadcast."

The wave-lengths in the gamma-ray spectrum are those between 4.100 and .005 Å. approximately.[10] The relative intensities of the waves represented have been studied by Skobelzyn,[11] Kohlrausch,[12] Ellis and Wooster,[13] Terrill and Pine,[14] J. Thibaud,[15] and others. The intensities shown in Fig. 4 are based on the studies by Skobelzyn.*

"The measurements made by Ellis and Wooster suggest a mean wave-length for the gamma-ray spectrum of 0.008 Å.U., corresponding to a voltage of 1,500,000 volts as indicated in

[10] Meyer u. Schweidler, p. 148, l. c.

[11] D. Skobelzyn, *Z. Phys.*, Vol. 43 (1927), p. 368.

[12] K. W. F. Kohlrausch, *Phys. Zeit.*, Vol. 28 (1927), p. 2.

[13] C. Ellis and W. Wooster, *Proc. Camb. Phil. Soc.*, Vol. 22 (1925), p. 853; Vol. 23 (1925–1927), pp. 717–729.

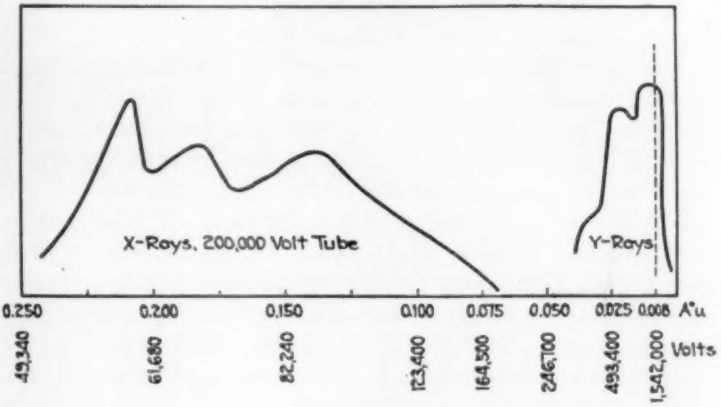
[14] H. M. Terrill and Mary Pine, *Jour. Cancer Research*, Vol. 8 (1924), p. 71.

[15] J. Thibaud, *Ann. d. Phys.*, Vol. 5 (1925), p. 119.

* It seems likely that the γ-ray spectrum, originating from an atomic disintegration, is not a "white" or continuous spectrum but a spectrum of discrete lines. As the ray emerges from the atom, however, it doubtless suffers a modification in wave-length owing to interaction with the radioactive substance itself and with any filter in its path, converting the separate rays through Compton scattering into a continuous spectrum. Skobelzyn determined the relative intensities of the different wave-lengths by the number of recoil electrons of given velocity (corresponding to a given wave-length of γ-ray) in a Wilson cloud chamber. His curve is "ironed out" in Fig. 4 to give a smoother curve than he observed and Fig. 4 therefore makes the intensity distribution appear more equable than it probably is. There is still some question of where the limits of the γ-ray spectrum lie. There is evidence of rays at wave-lengths greater than 0.040 Å.U., and some suggestion of rays around 0.002 Å.U. The limits of the γ-ray spectrum are therefore not defined in Fig. 4. In addition, Skobelzyn's determination of distribution of intensities rests upon only 160 observed recoil electron paths, a number insufficient to define the intensity distribution curve with high accuracy. The curve given in Fig. 4 may, however, be taken as approximately correct. The data from which it was plotted refer to γ-rays filtered through 3 mm. of lead.

Fig. 4,[1] somewhat shorter than the approximate mean wave-length to be derived from Skobelzyn's curve. On Fig. 4 is also represented the relative intensities found by Terrill and Pine [14] for the various wave-lengths in the X-ray spectrum as obtained from a 200,000 volt X-ray tube with a tungsten target, after filtering through 1 mm. of copper. It is to be noted that the γ-ray curve in Fig. 4 applies to γ-rays after filtering through 3 mm. of lead whereas the X-ray curve was taken with 1 mm. copper filter; this difference in filters exerts some distorting effect upon the true distribution curves, but

FIG. 4.



Curves showing intensities at various wave-lengths for x-rays (Terril and Pine) and for γ-rays (Skobelzyn). Dotted line represents the mean value for γ-rays. Given by Ellis and Wooster.

for the present purpose the curves given in Fig. 4 may be taken as a nearly true index of the distribution of wave-lengths in the two spectra.

"The areas under the intensity curves in Fig. 4 are entirely dependent upon the intensity of the source, whether it be an X-ray tube or a bulb or radium emanation. In Fig. 4 the two curves are drawn simply to give approximately equal heights." In the gamma radiations from mesothorium the longer wave-lengths of the spectrum are more strongly repre-

[1] Mehl, Doan and Barrett, l. c.

[14] L. c.

sented than in the radiation from radium, but as more and more radiothorium is generated by decomposition of mesothorium the proportion of short waves increases. The absolute intensity of radiation from a source is of course proportional to its mass; that is, the intensity of radiation from 2 grams of radium is twice that from 1 gram. The intensity of radiation from one milligram of radium after passing through 5 mm. of lead is one millicurie. That is the unit of radioactive intensity.

Besides the properties of penetrating opaque matter and registry on the photographic film, gamma rays produce ionization in gases through which they pass.

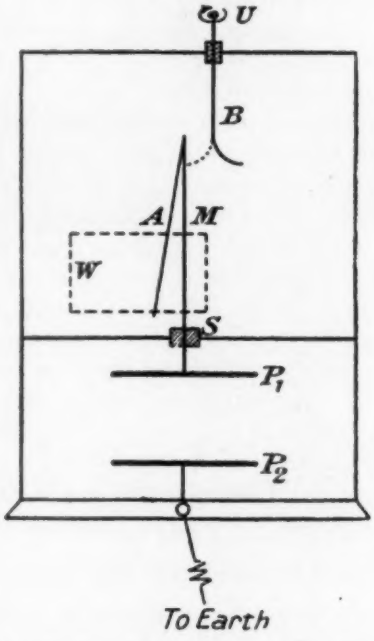
"*The Measurement of Radioactivity.*—In order to obtain a measure of the intensity of the radioactivity of a substance, the power of ionizing air possessed by the different rays is generally employed. A simple form of the apparatus used is shown in Fig. 5. It consists of two chambers constructed of metal, the upper one being known as the electroscope, and the lower one being known as the "ionization chamber."

"In the electroscope, an upright strip of stiff metal M is supported by a plug of sulphur, S, the sulphur serving to insulate the strip from the casing of the apparatus; at the upper end of the strip, a piece of very thin 'aluminium leaf,' A, is attached, the lower end of the leaf being free to fly outwards from the strip. The ionization chamber contains two horizontal plates of metal; the upper one, P_1, is connected, through the sulphur plug, to the metal strip M of the electroscope, whilst the lower one, P_2, is connected to 'earth.'

"The bent rod B, also insulated from the metallic casing, can be turned at will so as to touch the strip M. If, when B is in contact with the strip M, a rod of shellac, which has been electrified by rubbing on flannel, is brought into contact with the upper end U, both the strip and the aluminium leaf become electrified also. Owing to the mutual repulsion between like charges, the leaf flies outwards from the strip to a considerable angle; its exact position can be observed through a window (W) in the metal casing by means of a microscope provided with a micrometer scale. The rod B is then twisted again so that it no longer touches the strip.

"When once 'charged' in this way, the electroscope keeps its charge very well (so long as the ionization chamber contains no radioactive substance) and the leaf continues to point outwards at almost the same angle for many hours. There is, however, a small leakage of the charge to earth, either along the surface of the sulphur plug, or through the air, and consequently the leaf tends to drop slowly towards the strip *M*, the rate of movement being measured by means of the microscope.

FIG. 5.



Apparatus for measurement of radioactivity.

"If, on the other hand, a radioactive substance be placed on a little tray on the lower plate P_2, the air in the ionization chamber will become 'ionized,' and the rate of leakage of electricity from the upper plate to the lower plate (and thus to 'earth') increases very much, as is shown by the increased velocity at which the aluminium leaf falls. The rate of fall of the leaf, observed through the microscope, serves as a

measure of the activity of the substance placed in the tray. The instrument must first be 'standardized' by noting the rate of fall when some 'standard' radioactive substance, such as pure oxide of uranium, is placed in the ionization chamber; it can afterwards be used to examine the activity of unknown materials.

"For accurate work, various refinements may be introduced into the apparatus, but the simple form just described gives results sufficiently good for many purposes." [8]

Instead of air, a gas such as methyl iodide, used by the Braggs [16] in crystal analysis by X-rays, may be substituted. In this method [17] the current flowing between the electrodes is measured by a milliammeter.

The solid radioactive elements, both in cancer treatment and in radiography, are used in the form of chemical compounds in the powdered condition, usually as bromides or sulfates. A half gram (intensity 500 millicuries) of radium in this form is sealed in a glass or metal capsule approximately one-fourth inch in diameter. Radium emanation in contrast with all the other radioactive elements, is a gas, the heaviest of the series Helium, Neon, Argon, Krypton, Xenon, Radon, and like many other gases it can be adsorbed on activated carbon. In this highly condensed form 30 milligrams of the emanation can be concentrated [5] into a spherical container only 0.1 mm. in diameter (.1 mm. = 1/250 inch). The concentration of the source of rays as nearly as possible to a mathematical point is of importance in obtaining sharply defined radiographs, as will be seen in the subsequent discussion.

DECAY OF RADIOACTIVE SUBSTANCES.

Simultaneously with the emission of radiant energy described above radioactive substances change their identity by a process of spontaneous and continuous decay. Figure 3 above shows that Uranium "Y" becomes Actinium E, Mesothorium becomes Thorium E, and Ionium becomes Radium G, all three of which end-products are isotopes of lead. These processes of decay or transmutation originate in

[8] Evans, l. c., pp. 432–33.

[16] "Introduction to Crystal Analysis," D. Van Nostrand, 1929, p. 31, 57.

[17] A. H. Compton, *Phil. Mag.*, Vol. 41 (1921), p. 749.

the nuclei of the radioactive atoms. Radioactive processes do not obey the laws of chemical reaction but go on at a velocity which is entirely independent of temperature, of the presence or absence of solvents or catalysts, and even of the state of chemical combination in which the radioactive element exists. The rate of decay is such that in each interval of time the same fraction of the total original atoms present will decompose. Thus, if we start with a fresh preparation of uranium X_1 at the expiration of 24.6 days (see table above) one-half of the atoms of uranium X_1 have suffered change to uranium X_2, and one-half survive. At the end of a second 24.6 days, one-half of these surviving atoms have suffered change and one-quarter of the original atoms remain. At the end of a third period of 24.6 days, only one-eighth of the original number survive, etc. In this way, the actual rate of deterioration diminishes constantly and only in infinite time would all of the original atoms of uranium X_1 be decomposed.

For convenience, therefore, it is customary to speak of the "half-life period" of each element, that is, the period required for the intensity to drop to half the original intensity. It will be noted in Table I that the half-life period of radium is 1,645 years, that of uranium 5.5×10^9 years, of mesothorium I 6.7 years (commercial mesothorium contains about 30 per cent. radium and has a half-life period of 26 years,[5] while that of radium emanation is only 3.85 days and of radium C is 1×10^{-6} seconds. That is to say, for instance, that a gram of radium after 1645 years under whatever conditions or environment it has existed will have decayed to half a gram etc. The rate of decay is given by the equation:

$$\frac{I}{I_0} = e^{-\lambda t},$$

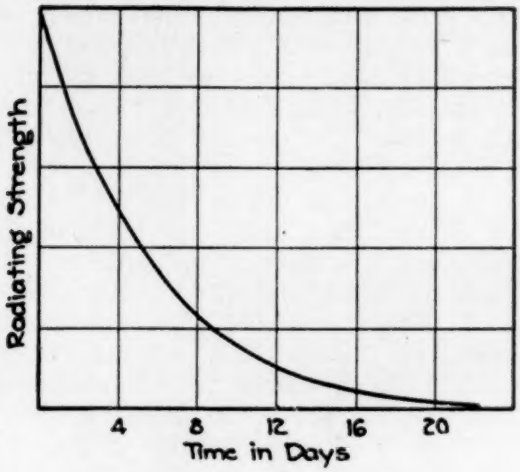
where I is the intensity measured after time t, I_0 is the original intensity and λ is the fraction of atoms which transforms in time t. The actual intensity after elapse of a time t is given by the equation:

$$\int_0^t I dt = I_0 \int_0^t e^{-\lambda t} dt = \frac{I_0}{\lambda}(1 - e^{-\lambda t}).$$

[5] Berthold and Riehl, l. c.

The decay curve for radium emanation is given in Fig. 6, and the numerical values in Table 2. The curve for the decay of commercial mesothorium is given in Fig. 7. The intensity of freshly prepared mesothorium rises during the first four years

FIG. 6.



Decay curve for radium emanation.

TABLE 2.

*Decay of Radium Emanation.**

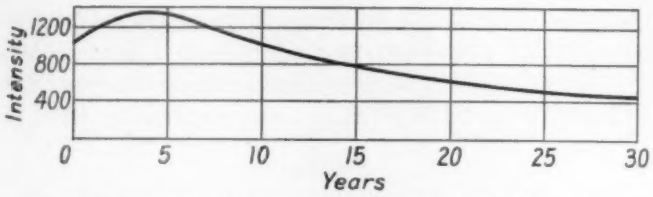
(for $\lambda = 0.007551$ hrs.; Half Life = 3.825 days)

Time t.	Intensity e^{-t}.	Time t.	Intensity $e^{-\lambda t}$.	Time t.	Intensity $e^{-\lambda t}$.	Time t.	Intensity $e^{-\lambda t}$.
0	1.0000						
1	0.9925	13	0.9064	25	0.8278	37	0.7560
2	0.9850	14	0.8996	26	0.8216	38	0.7504
3	0.9786	15	0.8929	27	0.8155	39	0.7448
4	0.9703	16	0.8861	28	0.8093	40	0.7391
5	0.9629	17	0.8795	29	0.8032	41	0.7336
6	0.9557	18	0.8729	30	0.7973	42	0.7282
7	0.9485	19	0.8662	31	0.7911	43	0.7225
8	0.9413	20	0.8597	32	0.7852	44	0.7171
9	0.9343	21	0.8533	33	0.7793	45	0.7117
10	0.9272	22	0.8468	34	0.7734	46	0.7064
11	0.9203	23	0.8405	35	0.7676	47	0.7010
12	0.9134	24	0.8343	36	0.7620	48	0.6960

* Meyer-Schweidler, "Radioactivität," Teubener, Berlin (1927) page 419.

after preparation of the compound due to the generation of radiothorium, and only after 10 years does the intensity of the gamma radiation begin to fall below its initial value.

FIG. 7.



Decay of γ-ray intensity in commercial mesothorium (Berthold and Riehl).

The rates of decay of radium and of mesothorium are so slow that for exposure times in radiography the decay during exposure may be entirely neglected. Emanation on the contrary decays so rapidly (half-life = 3.85 days) that corrections must be made for the change in intensity during an exposure. These corrections expressed in hours of exposure calculated from the above equation are given in Table 3.[2]

TABLE 3.

Comparison of Exposure Times in Hours for Radium and for Radium Emanation of Equal Initial Strength.

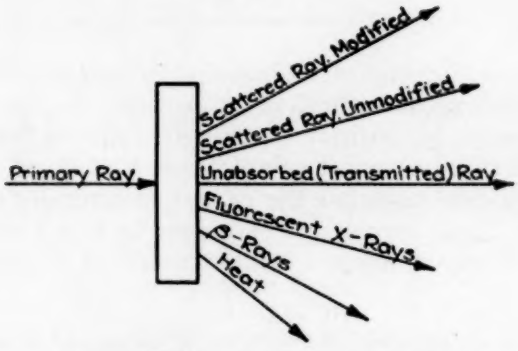
Radium.	Emanation.	Radium.	Emanation.	Radium.	Emanation.	Radium.	Emanation.
1	1.0	11	11.5	21	22.8	31	35.4
2	2.0	12	12.6	22	24.0	32	36.6
3	3.0	13	13.7	23	25.3	33	37.9
4	4.0	14	14.8	24	26.5	34	39.3
5	5.1	15	15.9	25	27.7	35	40.6
6	6.1	16	17.0	26	29.0	36	42.0
7	7.1	17	18.2	27	30.2	37	43.4
8	8.2	18	19.3	28	31.5	38	44.8
9	9.3	19	20.5	29	32.8	39	46.2
10	10.4	20	21.6	30	34.0	40	47.6

A less accurate method of calculating the average intensity of radium emanation over a given period of time is to assume that the rate of decay is linear with time. The errors in such calculations are negligible in practical radiography unless exposures are many hours in length.

SCATTERING AND ABSORPTION OF GAMMA RAYS BY METAL SECTIONS. PENETRATION.

The passage of a beam of rays through a section of metal, sometimes called a "filter," or "absorber," is not a simple process. The softer rays in the original beam being of less penetrating power, are filtered out selectively by absorption in the metal. The energy of the absorbed rays is transformed into heat within the filter, as indicated in Fig. 8. Another part of the energy of the original beam serves to eject electrons from the atoms of the filter.

FIG. 8.



Changes suffered by radiation in penetrating matter.

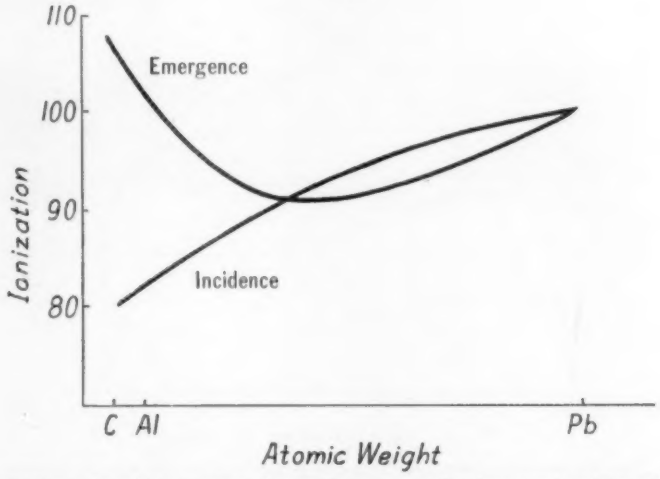
Scattering. "Definite measurements are available on the intensity of this secondary electron emission under various conditions. Eve [18] measured with an electroscope the intensity of the electron emission from sheets of different elements for gamma-rays filtered through 2 cms. of lead, separating the emission from the 2 sides of the sheets. The ionization in his electroscope due to electrons coming out of the side of the sheet upon which the gamma-rays were incident is plotted in Fig. 9 as the 'incidence' curve, while the curve marked 'emergence' gives the ionization caused by emission from the side of emergence of the gamma-rays.* The preponderance of emission in the

[18] A. S. Eve, *Philosophical Magazine of London, Edinburgh and Dublin*, Vol. 18 (1909), p. 275.

* No importance should be attached to the actual values of the ionization plotted here, for an unknown but constant amount must be subtracted from these values to correct for ionization from causes other than those here discussed.

emergence direction for light elements has its explanation in the quantum theories of radiation. As the atomic number of the screen is decreased, an increasing proportion of the secondary electrons is made up of electrons recoiling under the impact of the quanta of gamma-rays, which they deflect, or 'scatter.' These recoiling electrons obey the laws of conservation of energy and momentum in the impact and thus always go in the forward direction, in fact with gamma-rays nearly all proceed at an angle less than 45° from the direction of motion of the primary γ-ray. With the heavier elements, on

FIG. 9.



Ionization produced by secondary electrons from screens of various atomic weights when radiated by γ-rays from radium. (A. S. Eve.)

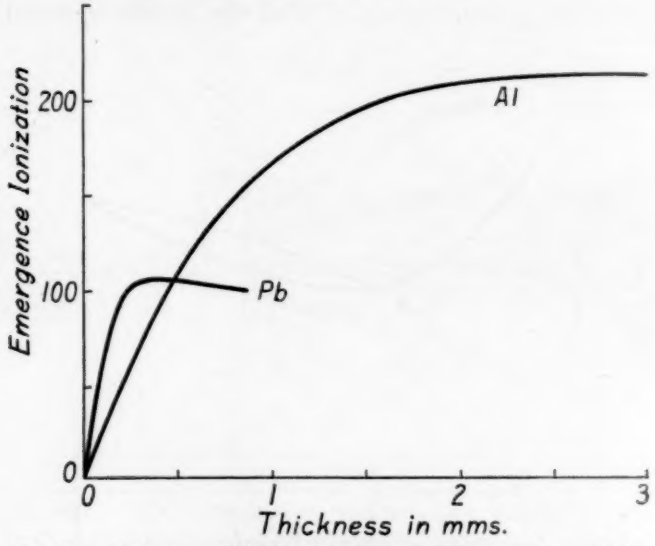
the other hand, the recoil electrons become few in number compared with photoelectrons. Photoelectrons result when the γ-ray quanta are absorbed instead of deflected, and have a distribution in space quite different from that of the recoil electrons so that the total emission from heavy elements is approximately the same from the incidence and emergence sides. The emergence ionization as a function of thickness of the sheet is plotted in Fig. 10 for aluminum and lead from measurements by Bragg.[19] The ionization increases with

[19] W. H. Bragg and J. P. V. Madsen, *Philosophical Magazine of London, Edinburgh and Dublin*, Vol. 16 (1908), p. 918.

thickness until the electrons starting at the incidence side of the sheet are unable to reach the emergence side; further increase in thickness simply reduces the intensity of the gamma-rays without adding to the number of electrons emitted." [2]

The ejection of these secondary electrons plays an important rôle in the photographic action involved in radiography, as will be described shortly.

FIG. 10.



Ionization on the emergent side of screens of various thicknesses when radiated by γ-rays from radium. (Bragg and Madsen.)

Another portion of the energy of the original beam is transformed into fluorescent X-rays in accordance with Stokes Law. This portion is also indicated in Fig. 8. These rays are usually absorbed before reaching the film.

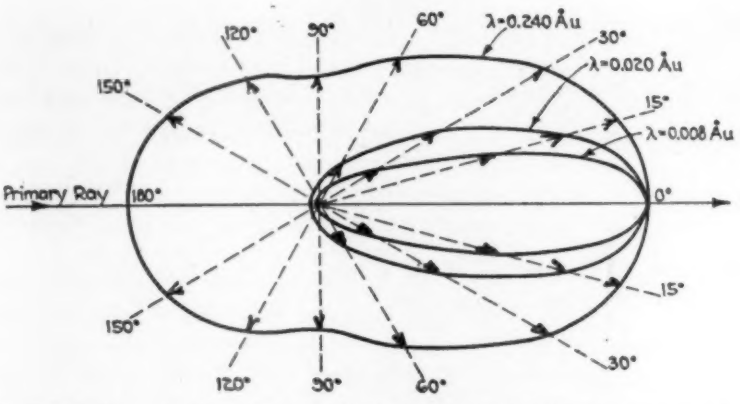
The intensity of the "scattered" radiation from an iron filter in the various space directions with reference to the primary ray is shown [17] in Fig. 11. The angular scattering

[2] C. S. Barrett, R. A. Gezelius, and R. F. Mehl, "The Technique of Radiography by Gamma Rays," *Metals and Alloys*, Dec. 1930, p. 872.

[17] A. H. Compton, *Philosophical Magazine of London, Edinburgh and Dublin*, Vol. 41 (1921), pp. 749–69. This distribution has a satisfactory explanation in the quantum theory of the Compton effect, the most recent form of the theory

of X-rays is shown for comparison with that of gamma rays in the same figure. This figure makes clear that the scattered gamma rays lie quite nearly in the same direction as the primary beam and therefore act *with* the primary beam in casting a rather sharp shadow of any flaw in its path, whereas the X-rays are more widely scattered and can reach the film by indirect routes and thus can fog or blur the outlines of the shadow of any flaw cast by the unchanged emergent beam. It is to screen out angularly scattered rays that the Bucky diaphragm, a series of lead slots or tubes near the film and parallel with the original beam, is used in X-ray radiography of the human body.

FIG. 11.



(Distribution of intensity of modified rays scattered at various angles with primary rays of wave-length 0.240 Å.U. (x-rays), 0.020 and 0.008 Å.U. (γ-rays).

The scattered rays undergo in addition to a change in direction, also a change in wave-length through the operation of the Compton effect. The Compton scattering is an increase in the wave-length of the primary beam, the amount of increase depending on the angle between the primary and scattered beam but being independent of the original wave-length. The increase in wave-length of a ray upon scattering

being that of Klein and Nishina (O. Klein and Nishina, Zeitschrift für Physik, Vol. 52 (1928), p. 398) which predicts the observed intensities accurately when a wave-length of 0.01 Å.U. is assumed for the γ-rays. For earlier theories see A. H. Compton, "X-Rays and Electrons," Van Nostrand Co. (1926), pp. 296–305.

may be comprehended as the result of an elastic impact of a wave quantum with a free electron. The electron absorbs energy and receives an impulse from the quantum with the result that the quantum, whose velocity c is invariable, changes its frequency ν. Before the impact the electron had mass m; suppose it had furthermore velocity v, energy and impulse zero. After the impulse the mass is $\frac{m}{\sqrt{1-\beta^2}}$, the impulse $\frac{mv}{\sqrt{1-\beta^2}}$, the energy $E_\beta = mc^2\left(\frac{1}{\sqrt{1-\beta^2}} - 1\right)$. The wave quantum before the impact had "mass" $\frac{h\nu_0}{c^2}$, impulse $\frac{h\nu_0}{c}$, energy $h\nu_0$; after the impact it had the corresponding magnitudes $\frac{h\nu}{c^2}, \frac{h\nu}{c}, h\nu$. The law of conservation of energy demands

$$h\nu_0 = mc^2\left(\frac{1}{\sqrt{1-\beta^2}} - 1\right) + h\nu. \tag{1}$$

The law of conservation of impulse, separated according to x and y directions demands

$$0 = -\frac{mv}{\sqrt{1-\beta^2}}\sin\varphi + \frac{h\nu}{c}\sin\theta \tag{2}$$

and

$$\frac{h\nu_0}{c} = \frac{mv}{\sqrt{1-\beta^2}}\cos\varphi + \frac{h\nu}{c}\cos\theta. \tag{3}$$

These three equations are insufficient to determine the four unknowns $v = \beta c$, ν, φ, θ. The radiation may be scattered in any direction without fixing the above assumptions, and determining the direction. For a chosen direction of scattering θ, however, the other unknowns are determined. From these considerations Compton and also De Bye have shown (Kohlrausch, l. c.) that the change in wave-length upon scattering is

$$\Delta\lambda = 24.2 \times 10^{-11}(1 - \cos\theta) \text{ cm}.$$

This equation permits calculation of the intensity of scattered radiation at various angles to the primary beam for a given wave-length, as well as the change in wave-length due to scattering at any given angle. A beam scattered from iron at 90 degrees to the primary beam increases in wave-length by .024 Å. due to the Compton effect. This increase in the case of an X-ray wave of .15 Å. original length is relatively slight, about 16 per cent. But for a gamma ray of wave-length .01 Å. it is an increase of more than 200 per cent. These relatively soft (long) gamma rays resulting from scattering will be absorbed in the filter before reaching the film to an extent proportional to their tripled wave-length, and being thus largely absorbed will therefore be of correspondingly little effect in blurring the outlines of any flaw in the filter, within the time of exposure necessary for the unchanged and more penetrating original beam to register on the film.

"Knowledge of these factors enables a calculation to be made of the area of film which will be fogged by the scattering from a given point in a specimen being radiographed. Taking Compton's measurements [17] Barret [2] has estimated the relative intensity of scattered gamma-rays reaching different points at the rear of a lead sheet 1 inch thick, assuming a small element of volume on the radium side of the sheet as the only source of scattering. Because of the distribution-in-angle of intensities, the softening upon scattering, the different thicknesses traversed by the scattered rays and the weakening according to the square of the distance from the source of scattering, this element of volume can cause fogging over a circle only an inch or so in diameter, as shown in Fig. 12. (The intensity of the primary ray is not indicated in the figure.) While this example represents a simplified case not found in practice, it is sufficient to show an important point; namely, that with gamma rays a specimen of great length and breadth can be radiographed with practically no more fogging from scattering arising in the specimen than one would experience with a small specimen of the same thickness.

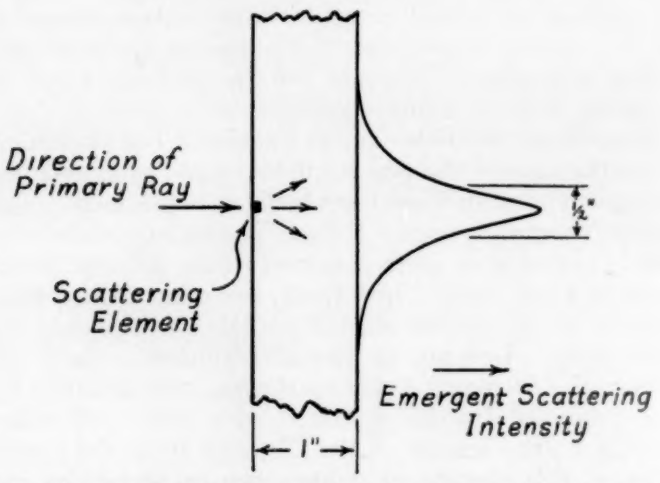
"For best results films should be shielded from scattered rays reaching them from the rear (originating in the walls and

[17] A. H. Compton, *Philosophical Magazine of London, Edinburgh and Dublin*, Vol. 41 (1921), p. 749.

floor of the room), although the amount of this is not excessive. Figure 13 taken through 4 inches of steel had part of the film protected from this rear scattering by an 1/8 in. thickness of lead; another part was protected by a 1/4 in. thickness. As is seen the fog was reduced by the 1/8 in. sheet, but no further reduction was effected by the additional 1/8 in." [2]

The practical significance of scattering in radiography by gamma rays and by X-rays is nicely illustrated in the experiment of Barrett and Gezelius.[20] Figure 14 shows an X-ray

FIG. 12.



Distribution of intensity near the back of a lead block, of γ-rays scattered by an element of volume in the front surface of the block. (Barrett, Gezelius and Mehl.)

radiograph of a small, heat-resisting casting, taken without precautions to exclude scattered rays from reaching the film. It exhibits extensive halation from the scattering effect. Figure 15 is a gamma ray radiograph of the same casting, taken under the same conditions. Halation due to scattering is almost entirely absent. The meaning of this result for the industrial radiographer is that little or no precaution against scattering need be used with gamma rays.

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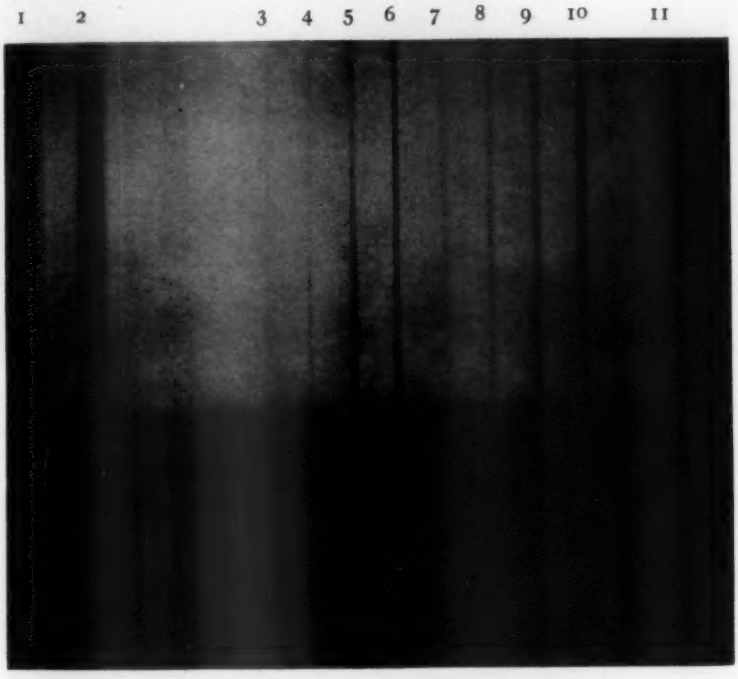
[2] Barrett, Gezelius, Mehl, l. c.

[20] R. F. Mehl, *Trans. Amer. Iron and Steel Inst.*, 1932.

Finally, a part of the original beam passes through the filter without absorption, scattering or modification.

Absorption. In general the absorption coefficient of X- and gamma rays increases with the atomic number of the absorbing atoms, the number of atoms per unit volume (there-

FIG. 13.



3

Radiograph of slotted steel plate arranged as shown in Fig. 1. Total thickness 4″; distance, source to film, 18″; exposure 3050 milligram hours using 3 lead intensifying screens with 2 films; dimensions of slots as follows from left to right:

Width in mms.....	6	1	1	1	1	2	2	2	2
Depth in mms.....	13	2	3	6	12	2	3.5	6.5	12

fore with the density, approximately), and the wave-length of the rays. Kohlrausch has shown that most of the measurements of absorption of gamma rays in iron and other metal filters have no general validity because the scattered radiations originating in the filter are partially but not completely eliminated in measuring the intensity of the emergent beam.

FIG. 14.

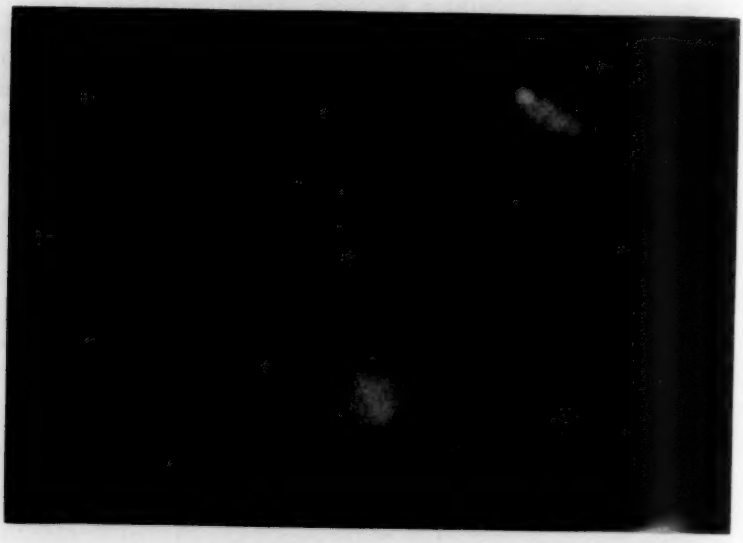
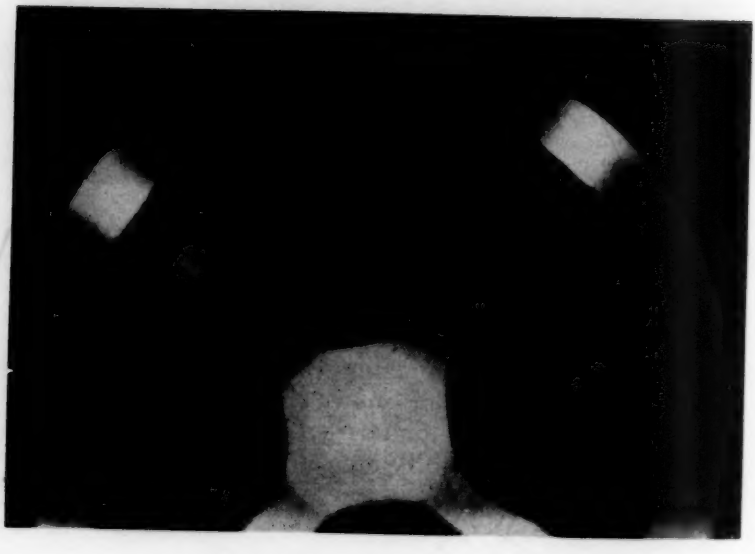
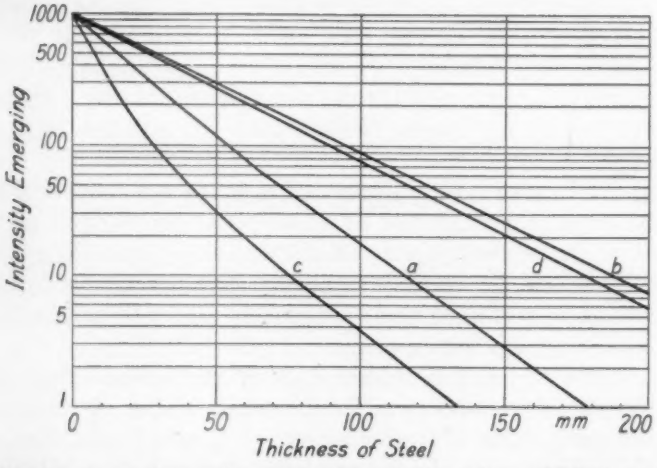


FIG. 15.



The results so obtained do not apply either to the case of an infinitely narrow beam of rays with none of the secondary rays included, nor to the case of a broad beam of rays in which the scattered radiation is included in the measurement. The second case is, of course, the one involved in radiography, where the film is fastened tightly to the back of the filter (test object) and thus exposed to the scattered radiation as well as to the unchanged beam. For the former case, that is, an infinitely narrow beam with all secondary rays excluded, Kohlrausch finds for two wave components in the complex spectrum of gamma rays from radium, linear absorption coefficients μ in iron of 0.356 and 0.632 cm.$^{-1}$. On the basis of these values Berthold and Riehl [5] have calculated the intensity of the emergent beam after passing through steel filters of various thicknesses. Their results are shown at *a* in Fig. 16.

FIG. 16.



Absorption of γ-rays from radium and from mesothorium in iron. (Berthold and Riehl.) *a*, radium, narrow beam, scattered rays excluded. *b*, radium, broad beam, scattered rays included. *c*, mesothorium, narrow beam, scattered rays excluded. *d*, mesothorium, broad beam, scattered rays included.

For a broad beam of rays (second case above) as is used in radiography, the absorption coefficient is smaller, since with increasing volume of the filter the effect of scattered radiation

[5] Berthold and Riehl, l. c.

as compared with the undeflected beam increases. Based on measurements of M. Blau [21] with a zinc filter they calculate an absorption coefficient, for gamma rays from radium in an iron filter, of 0.225 from which value the curve *b* in the above mentioned figure is calculated for radiographic conditions.

In a similar manner the absorption coefficient of the softer gamma rays from mesothorium was calculated for both cases. Using the wave-lengths and intensities of mesothorium as measured photoelectrically, and calculating the absorption coefficient of each wave-length in an iron filter from Kohlrausch's equation for the coefficients of the photoelectrically active wave-lengths of radium's gamma rays, curve *c* was obtained. This curve lies lower (absorption greater) than all the others because, as pointed out previously, in mesothorium the long wave components of the spectrum are more strongly represented (and more completely absorbed) than in radium's gamma rays. The presence of these longer wave-lengths would, it should be noted, increases the sensitivity of flaw detection in the case of thin filters (test objects) because of their relatively greater absorption coefficient. The wave-lengths from mesothorium may therefore be considered as intermediate between X-rays and the gamma rays from radium.

In the case of a broad beam of rays from mesothorium one would expect a decrease of absorption coefficient as compared with narrow beam conditions, corresponding to the decrease found for radium's gamma rays. Curve *d* gives the experimental results with mesothorium under these conditions; and confirms this expectation.

In some earlier experiments Soddy and Russell [22] measured absorption of gamma rays from radium under conditions approximating those of radiography. "It was found that a ray of original intensity I_0 after passing through a metal filter of thickness x and density ρ was

$$I = I_0 e^{-\mu x} = I_0 e^{-(\mu/\rho)\rho x},$$

where μ is the linear absorption coefficient and (μ/ρ) is the

[21] M. Blau, Wiener Berichte, Vol. 127 (1918), p. 1253.

[22] Soddy and Russell, *Phil. Mag.*, Vol. 18 (1909), p. 620. Mr. and Mrs. Soddy and A. S. Russell, *Phil. Mag.*, Vol. 19 (1910), p. 725.

mass absorption coefficient. Some values for these coefficients are given in Table 4. Soddy and Russell found a constant

TABLE 4.

Absorption Coefficient for γ-rays from Radium According to Soddy and Russell.

Metal.	μ(cm.$^{-1}$).	μ/ρ.
Fe	0.304	0.0399
Cu	0.351	0.0398
Brass	0.325	0.0389
Al	0.111	0.0401
Pb	0.495	0.0434

value of μ in lead from 2 to 22 cm. thick, and similarly found constant values over large ranges of thickness in other materials. Later observers were able to detect a hardening of the rays as they penetrated greater thicknesses, caused by the more rapid absorption of the softer components, and resulting in a progressive decrease in μ with increasing thickness, an effect doubtless hidden from the earlier investigators by the presence of considerable scattering in their work. Taking the simple exponential equation above as representing absorption in radiography, the exposure time required to give a certain density on a photographic film behind x cms. of metal varies with the thickness according to the relation

$$t = t_0 e^{\mu x} = t_0 e^{(\mu/\rho)\rho x},$$

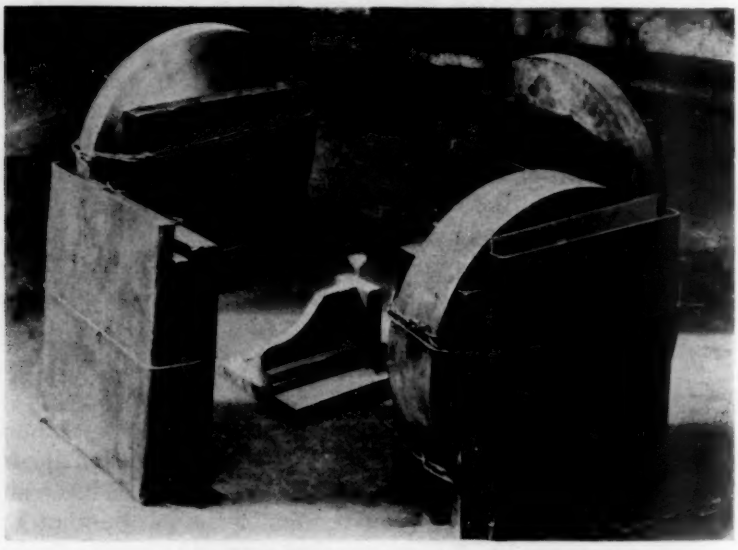
where t_0 is the time required with $x = 0$. The derivation of this equation assumes (1) that a constant source of intensity is used, (2) that the effect on a film is the product of intensity times time, a valid assumption, and (3) that the film is completely protected by lead shields from scattered rays reaching it from the rear (excited by primary rays striking the walls, floor, etc.).

"As there was some doubt as to just how well the above formula and values of Table 4 would serve to establish radiographic exposure times" the American investigators determined the exposure times (intensity of emergent beam) "under actual conditions."

"Steel plates 12 in. square and steel disks 16 in. in diameter were arranged about a holder of radium emanation as shown in Fig. 17. Films were shielded from gamma-rays scattered from

walls and floor by lead sheet 1/8 in. thick shown tied to the back of the piles of plates. Various slots and holes were cut in the plates *next to the emanation* (not next to the film) to test the thickness of defect that could be detected. A distance of 18 in. from source to film was used throughout the test. With a given thickness of plates and type of intensifying screen two exposures were made and the density measured at points on the films corresponding to the full thickness of metal. The

FIG. 17.



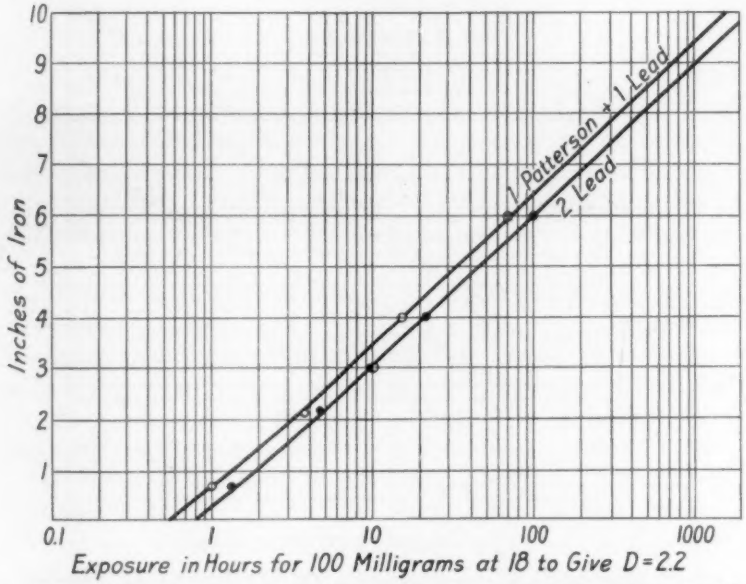
Small group of thick objects in γ-ray testing.

exposure times actually used were transformed to the times that would be required for corresponding exposures with 100 milligrams of radium. Plotted on semi-logarithmic paper these times and densities, for a given thickness, gave two points on a curve of the type shown in Fig. 18. These points located the time-density curve with sufficient accuracy since only the position of the curve along the time axis, not its shape, is affected by the thickness of the absorbing plates. The time required to give any desired degree of blackening could then be read from the curve. Such exposures were

used that the desired point lay either between the two used, or very close to one." [2]

Thus the absorption results of Barrett, Gezelius and Mehl show not the absolute intensity of the emergent beam but its relative intensity expressed in terms of exposure time necessary to blacken a film on which the emergent beam falls. These results are shown at *e* in Fig. 19 for various thicknesses of iron filters. They are to be compared with those of

FIG. 18.



Early experiments. Exposure times for radiography with γ-rays from radium. Conditions assumed: Constant source of 100 milligrams at 18" from film using intensifying screens indicated. Resultant density (with overdevelopment) suitable for simultaneous viewing of two films. (See also Fig. 24.)

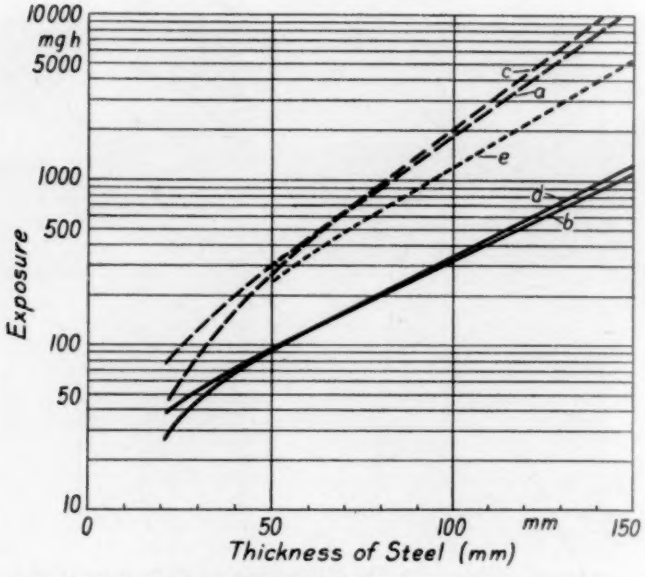
Berthold and Riehl shown at *b* in Fig. 19. Their slopes are quite similar. The difference in location of the lines is due primarily to the quality of film, and to the thicker intensification screens used in the later experiments, as will be described under "Photographic Action."

The experimental results of Barrett, Gezelius and Mehl

[2] Barrett, Gezelius and Mehl, l. c.

and the calculated results of Berthold and Riehl with respect to the absorption coefficient of gamma rays from radium in an iron filter thus agree quite satisfactorily. The coefficient arrived at for absorption of these rays in an iron filter under radiographic conditions is about. 225 cm.$^{-1}$, which is very substantially less than the .356 and .632 determined by Kohlrausch for narrow beam conditions, and therefore more favorable to the rapid testing of iron objects.

FIG. 19.



Exposure times for steel; Agfa Spezialroentgenfilm with two Sinegran screens, 30 cm. distance between source and film. Blackening of unexposed film = 1.0. (Berthold and Riehl.) *a*, radium, narrow beam. *b*, radium, broad beam. *c*, mesothorium, narrow beam. *d*, mesothorium, broad beam. *e*, radium according to Barrett, Gezelius, Mehl.

Since the gamma radiation from radium emanation or Radon is really that of its decomposition product Radium C it does not require separate discussion.

(*To be concluded.*)

A GRAPHICAL REPRESENTATION OF TWO-DIMENSIONAL STRESS DISTRIBUTIONS.*

BY

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INTRODUCTION.

A problem, which often confronts the engineer, is that of choosing a simple but adequate system of representing a two-dimensional state of stress. In general the solution of a problem in plane stress, whether obtained analytically or experimentally, yields a knowledge of the magnitudes and directions of the principal stresses at points throughout a certain stressed region. Graphical methods of representing the results have been found to be more illuminating than a mere tabulation of values. The most successful of the former has been the delineation of *directions* by means of a system of isostatic lines (stress trajectories). However, no similarly simple arrangement has been found for representing the *magnitudes* of the stresses.

A common procedure, with regard to magnitudes, is to select certain critical sections along which stress values are plotted as ordinates. Often the number of such sections, required for an adequate treatment, is so great that the diagram becomes very complicated. If an attempt is made to simplify the figure by eliminating certain sections, regions in which the stresses are important are likely to be omitted.

An alternative method is founded on the use of stress contours—lines along which the magnitudes of the principal stresses are constant. In this way a complete stress representation may be obtained; but only through the medium of three diagrams: one network of isostatics and one contour diagram for each principal stress.

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* Published in "Le Génie Civil," Juin 20, 1931.

In the method presented below, a complete portrayal of any stress distribution is given by two diagrams. One of these is the usual isostatic network and the other is a system of curves representing the magnitudes of forces normal to the isostatics.

THEORY.

If the principal stresses ν_1 and ν_2, along a series of isostatic lines, are known, it will be shown that the magnitudes of the stresses may be represented by a vector diagram of the forces acting on and normally to the isostatics.

Let each elementary length ds_1, of an isostatic, be represented in the vector diagram by a vector $d\sigma_1$ whose direction is normal to that of ds_1 and whose magnitude is equal to the product of ds_1 and the principal stress normal to it:

$$d\sigma_1 = \nu_2 ds_1. \tag{1}$$

For the orthogonal set of isostatics we have a similar relation

$$d\sigma_2 = \nu_1 ds_2. \tag{2}$$

The reader will recognize the analogy between the vector diagram and the force polygon of graphical statics. In a like manner the isostatic corresponds to the equilibrium (funicular) polygon.

The normal or tangential stresses on any section may be deduced from a consideration of the equilibrium of an elementary triangle omn, as shown in Fig. 1, in which $mn = ds_2$ and $no = ds_1$. The force triangle (Fig. 2) corresponding to Fig. 1 is constructed by drawing $(m)(n)$ perpendicular to mn and equal to $\nu_1 ds_2$; and, similarly, $(n)(o)$ is perpendicular to no and equal to $\nu_2 ds_1$. The resultant of $\nu_1 ds_2$ and $\nu_2 ds_1$ is given by $d\sigma$. If ν is the normal stress on any inclined section ds and ω is the angle between $d\sigma$ and the normal to ds, then

$$\nu ds = d\sigma \cos \omega,$$

or

$$\nu = \frac{d\sigma}{ds} \cos \omega. \tag{3}$$

In an analogous way the tangential stress is found to be

$$\tau = \frac{d\sigma}{ds} \sin \omega. \tag{4}$$

FIG. 1.

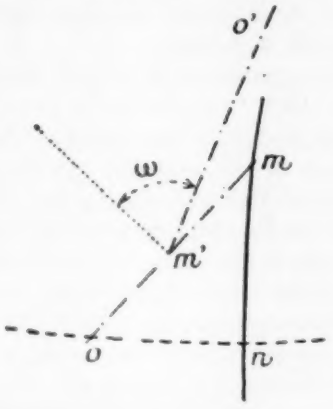


FIG. 2.

It should be noted that body forces have been neglected in this derivation. The full significance of the relations so far established will be brought out by a study of Figs. 3 and 4. We are here concerned with a region bounded by isostatics and acted upon by lateral and longitudinal forces.

I′I′ is the vector diagram of the forces acting on isostatic II.

II′II′ is the vector diagram of the forces acting on isostatic II II.

abcd is the equilibrium polygon of all the forces acting on the body.

FIG. 3.

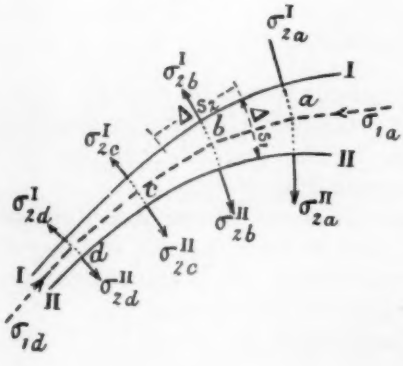
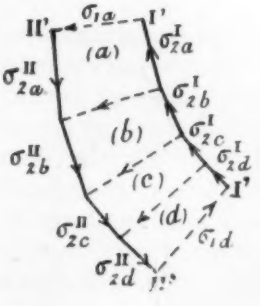


FIG. 4.



It may be noted that, as II II and I I are taken more and more closely together, the equilibrium polygon approaches the ultimate common isostatic as a limit.

That the force diagram represents the stress distribution uniquely follows from the fact that for each point in the isostatic system there is one and only one point in the vector system; and for each section in the isostatic system there is a single corresponding curve in the vector system. It should be observed, however, that an element $d\sigma$ of a force curve is, in general, inclined at some angle ω to the normal of the corresponding element ds in the isostatic system.

The tangents to the force-curves are parallel to normals to the isostatics at corresponding points. This condition leads to an interesting relationship between the two sets of curves. If we let φ be the azimuth of the tangent to an isostatic and φ' the azimuth of the tangent to the force-curve at the corresponding point, then

$$\varphi' = \varphi + 90°.$$

As $\Delta\varphi = \Delta\varphi'$, from equations (1) and (2) we obtain:

$$\frac{d\sigma_1}{d\varphi'} = \nu_2 \frac{ds_1}{d\varphi},$$

from which

$$r_2' = r_2\nu_2. \tag{5}$$

Similarly

$$\frac{d\sigma_2}{d\varphi'} = \nu_1 \frac{ds_2}{d\varphi},$$

from which

$$r_1' = r_1\nu_1. \tag{6}$$

The two diagrams are thus connected by a simple relation: the radius of curvature of a force-curve is equal to the product of the radius of curvature of the isostatic and its normal principal stress at the corresponding point.

Frequently the determination of stress distributions is accomplished analytically, and in such cases it is convenient to arrive at the force-vector system via a transformation to a system of curvilinear orthogonal coördinates.

The transformation equations are:

$$\eta + \eta_e = \int_{x_0}^{x} \nu_y dx = -\int_{y_0}^{y} \tau dy, \tag{7}$$

$$\xi + \xi_e = \int_{y_0}^{y} \nu_x dy = -\int_{x_0}^{x} \tau dx, \tag{8}$$

in which ν_x and ν_y are the stresses parallel to X and Y, the axes of the original system of rectangular cartesian coördinates; τ is the corresponding shear; x_0 and y_0 are the coordinates which fix the isostatic system in space; ξ and η are the force vectors parallel to X and Y respectively; ξ_e and η_e are the X and Y components of the external forces.

Although the transformation is orthogonal it is not, in general, conformal. Hence a geometric figure in the original system is not transformed into a similar shape in the curvilinear system. The loci x = constant, y = constant are represented in the force diagram respectively by $f(\xi, \eta)$ = constant and $\psi(\xi, \eta)$ = constant, according to the inverse transformation,

$$x = f(\xi, \eta) \qquad \text{and} \qquad y = \psi(\xi, \eta). \tag{9}$$

But, although the lines x = constant and y = constant intersect at right angles, their counterparts in the curvilinear system, $f(\xi, \eta)$ = constant and $\psi(\xi, \eta)$ = constant, are curves whose corresponding tangents intersect, in general, at an angle which varies from point to point.

APPLICATION.

The representation of an analytical solution will serve to illustrate the method. The example chosen is the well known case of a circular disk under diametral compression.

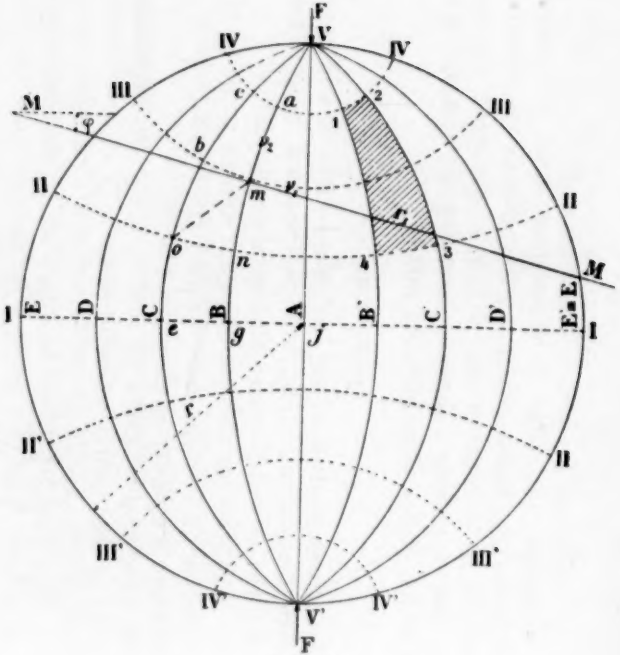
The isostatics for this problem are known to be given by two families of circular arcs formed by stereographic projection from a sphere, as shown in Fig. 5.

The principal stresses ν_1 and ν_2 may be expressed as functions of the radii of these circles:

$$\nu_1 = \frac{F}{\pi r}\left[1 - \left(\frac{r}{r_1}\right)^2\right],$$

$$\nu_2 = -\frac{F}{\pi r}\left\{1 - \left(\frac{r}{r_1}\right)^2 + 2\sqrt{\left[1 - \left(\frac{r}{r_1}\right)^2\right]\left[1 + \left(\frac{r}{r_2}\right)^2\right]}\right\}.$$

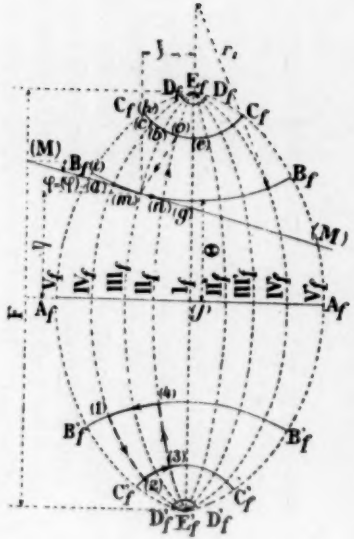
Fig. 5.



Isostatics.

Circular disk under diametrical compression.

Fig. 6.



Force diagram.

The directions of ν_1 and ν_2 are tangential to the dotted and full line circles respectively; r_1 is the radius of a meridian circle and r_2 is the radius of the orthogonal circle; the radius of the disk is designated by r and F is the applied force.

Since ν_1 is constant along meridian circles, it follows from equation (6) that the corresponding force-curves are also circular arcs of radius.

$$r_1' = r_1\nu_1 = \frac{Fr_1}{\pi r}\left[1 - \left(\frac{r}{r_1}\right)^2\right].$$

Consider the point of intersection, m, of meridian circle B and circle III–III. The direction of the principal stress ν_1 at this point is given by M–M, making an angle φ with the horizontal. In the force-diagram (Fig. 6) the corresponding vector $d\sigma_2 = \nu_1 ds_2$ has a direction (M)–(M), so that

$$(\varphi) = \varphi.$$

Replace the infinitely small functions by their finite values:

$$mn = \Delta s_2,$$

$$(m)(n) = \nu_1 \Delta s_2 = \Delta\sigma_2,$$

where $(m)(n)$ is perpendicular to mn; and, similarly

$$no = \Delta s_1,$$

$$(n)(o) = \nu_2 \Delta s_1 = \Delta\sigma_1.$$

The stress on an inclined face such as om is given by the ratio of $(o)(m)$, measured in units of force, to the length om. The normal and tangential components are then found from the relations of equations (3) and (4), where ω is the angle between $(o)(m)$ and the normal to om.

A simple test will show that the force diagram satisfies the rules of graphical statics. Consider the cross-hatched area 1, 2, 3, 4 (Fig. 5) as a free body in equilibrium under the action of the principal stresses acting along the sides 1–2, 2–3, 3–4, 4–1. The resultant forces are represented in the force diagram by chords (1)–(2), (2)–(3), (3)–(4), (4)–(1). The latter are correctly consecutive and form a closed figure.

Additional interesting relations between the isostatic and force diagrams are given in the following table.

Isostatic Diagram.	Force Diagram.
Force (F) applied at V and V′	Vector F.
Arcs A, B, C, D, B′, C′, D′	Arcs A_F, B_F, C_F, D_F, B_F', C_F', D_F'.
The contour of the disk	Points E_F and E_F'.
Arcs I, II, III, IV, II′, III′, IV′	Curves I_F, II_F, III_F, IV_F, II_F', III_F', IV_F'.
Point-circles V and V′	Curves V_F and V_F'.

MEDAL DAY MEETING.

The annual Medal Day Meeting of The Franklin Institute was held in the Hall of the Institute at three-thirty o'clock on the afternoon of Wednesday, May seventeenth.

This meeting which was also the regular monthly meeting of the Society was called to order by the President, Mr. Nathan Hayward.

Upon motion, the minutes of the preceding monthly meeting were approved as printed in the JOURNAL OF THE FRANKLIN INSTITUTE.

There being no other business, the Honorary Membership and Medals were then bestowed. The awards recommended by the Committee on Science and the Arts during the Institute year were given at this time.

AWARD OF CERTIFICATE OF HONORARY MEMBERSHIP.

To Mr. Alfred Rigling, Philadelphia, Pennsylvania. Dr. Howard McClenahan, sponsor.

The President called upon Dr. McClenahan.

Dr. McClenahan: "Mr. President, with the wholehearted approval of the Board of Managers of The Franklin Institute, I have the honor to present for election to honorary membership in The Franklin Institute, Alfred Rigling, Librarian and Assistant Secretary of the Institute 'in recognition of a half century of helpful and intelligent service, during which long term of duty well done, he has been a pillar of strength to the Institute, a source of comfort to the discouraged and of knowledge to the ignorant, a librarian amazingly informed concerning his library and a kindly gentlemen skillful and eager in friendly service to his fellow-men.'"

The President presented the Certificate as follows: "To you, Mr. Rigling, faithful for fifty years in the care and upbuilding of our great library, I present this Certificate of Honorary Membership. We are all delighted to have this honor come to you."

Mr. Rigling expressed his gratitude for this mark of esteem by The Franklin Institute.

AWARD OF THE CERTIFICATES OF MERIT.

To Henry Selby Hele-Shaw, D.Sc., LL.D., F.R.S., London, England. Dr. T. K. Cleveland, sponsor.

The President called upon Dr. Cleveland.

Dr. Cleveland: "Mr. President, I present, in absentia, Dr. Henry Selby Hele-Shaw, to receive a Certificate of Merit 'in consideration of his development of a superior filtering device involving stream line principles.' Dr. Hele-Shaw is represented by Mr. Watson, the Consul for Great Britain in Philadelphia."

The President presented the Certificate as follows: "Mr. Consul, it is a great honor to hand to you this Certificate of Merit which I would ask you to forward to your distinguished countryman, Dr. Hele-Shaw."

In accepting this award the Consul replied: "Your perennial medalist (by proxy) is always honoured to appear at this illustrious gathering to accept in behalf of a compatriot, one of the highly prized awards yearly issued by The Franklin Institute to distinguished men of science the world over and at the same time he feels bound to point out with no avaracious intent, that he is a strong believer in the gold standard and it is very suitable on that account that one of your medals is being presented today to a distinguished scientist from France. He hopes that his country will be back on the gold standard by next May.

"He now regrets that when at Cambridge over thirty years ago, he did not have sufficient foresight to study some scientific subject in lieu of classics as he would have today been better equipped to play the pleasant part assigned to him on these occasions.

"He has much pleasure in accepting this award on behalf of his countryman, Dr. Hele-Shaw."

To Mr. Arthur F. Poole, Ithaca, New York. Mr. C. E. Bonine, sponsor.

The President called upon Mr. Bonine.

Mr. Bonine: "Mr. President, I wish to present Mr. Arthur F. Poole to receive a Certificate of Merit 'in consideration of his combination in a battery operated clock, of known mechanisms, that has produced a clock in which the swing of the pendulum is the driving force of the gears controlling the

hands, and also determines the frequency of the impulse, that requires battery renewal at long intervals only, and that is an accurate time-keeper.'"

The President presented the Certificate as follows: "In accordance with this recommendation, I present to you this Certificate of Merit."

Mr. Poole expressed his appreciation of this presentation.

AWARD OF LONGSTRETH MEDALS.

To Mr. Howard L. Ingersoll, New York Central Lines, New York City, New York. Mr. Benjamin Franklin, sponsor.

The President called upon Mr. Franklin.

Mr. Franklin: "Mr. President, I have the honor to present to you, upon the recommendation of the Committee on Science and the Arts, Mr. Howard L. Ingersoll as the recipient of an Edward Longstreth Medal 'in consideration of his development of the locomotive booster to a state in which it gives valuable aid to locomotive performance and railroad service.'"

The President presented the Medal as follows: "Mr. Ingersoll, it is a great pleasure to me, as President of The Franklin Institute, to present to you this Edward Longstreth Medal."

Mr. Ingersoll responded: "I greatly appreciate this honor. I think very highly of the name of the founder of this wonderful institution which has grown up. I appreciate this very great honor which has come to me."

To Dunlap Jamison McAdam, Jr., Ph.D., Bureau of Standards, Washington, D.C. Mr. G. H. Clamer, sponsor.

The President called upon Mr. Clamer.

Mr. Clamer: "Mr. President, it is my privilege to present Dr. Dunlap Jamison McAdam, Bureau of Standards, Washington, D. C., to receive a Longstreth Medal 'in consideration of the fact that Dr. McAdam has provided information that satisfactorily explains certain structural failures, has developed formulæ and diagrams to illustrate the complex relationship of the influences of stress on corrosion and has done more than anyone else to establish the fundamental principles of corrosion fatigue, coupled with the fact that the information

provided has already been usefully applied and should have wide future practical application.'"

The President: "In the name of The Franklin Institute, upon the recommendation of its Committee on Science and the Arts, I present to you the John Price Wetherill Medal."

Dr. McAdam expressed his deep appreciation of the award of this honor to him.

AWARD OF WETHERILL MEDALS.

To Messrs. Henry S. Hulbert, Francis C. McMath and Robert R. McMath, the McMath-Hulbert Observatory, University of Michigan, Lake Angelus, Michigan. Mr. James Stokley, sponsor.

The President called upon Mr. Stokley.

Mr. Stokley: "Mr. President, upon the recommendation of the Committee on Science and the Arts, a John Price Wetherill Medal has been awarded to Messrs. Henry S. Hulbert, Francis C. McMath and Robert R. McMath 'in consideration of their design and construction of novel apparatus for the making of motion pictures of astronomical subjects, which have proven of value in the teaching and popularization of astronomy.' I present these gentlemen to to you."

The President presented the Medal as follows: "Mr. McMath, upon the recommendation of the Committee on Science and the Arts as just given, I have the honor to present to you a John Price Wetherill Medal for work which has been done by yourself, your father and your associate, Judge Hulbert."

Messrs. Hulbert, McMath and McMath expressed their thanks for the honor.

To Industrial Brownhoist Corporation, Bay City, Michigan. Mr. R. L. Wood, sponsor.

The President called upon Mr. Wood.

Mr. Wood: "Mr. President, the Board of Managers of The Franklin Institute, upon the recommendation of its Committee on Science and the Arts, has voted to award to the Industrial Brownhoist Corporation, a Wetherill Medal 'in consideration of the high degree of ingenuity in design and execution of detail, embodied in a successful machine for

cleaning railway ballast, resulting in a real contribution to railroading and the solution of a maintenance problem of great moment, especially under traffic conditions of extreme density.' I am happy to present Mr. Alexander C. Brown, who will receive the medal in behalf of his company."

The President: "Mr. Brown, upon recommendation of the Committee on Science and the Arts as just given, I have the honor to present to you, the representative of the Industrial Brownhoist Corporation, a John Price Wetherill Medal."

In accepting the award Mr. Brown replied "Mr. President, on behalf of my Company, I wish to express to The Franklin Institute our sincere appreciation of this high honor. In these trying days, the rewards of industry are few and far between, and very small. Anyone who aids in keeping us cheerful and hopeful renders a great service. The award of this medal is indeed in the nature of a 'pat on the back!' Its welcome expression is most encouraging."

To The Koppers Company of Delaware, Pittsburgh, Pennsylvania. Mr. O. B. Evans, sponsor.

The President called upon Mr. Evans.

Mr. Evans: "Mr. President, it is my privilege to report to you that the Committee on Science and the Arts has recommended the award of a John Price Wetherill Medal to the Koppers Company 'in consideration of the development of systems for the liquid purification of gases, the success of which is evidenced by the number of such installations in regular use.' I wish to present Mr. F. W. Sperr, Jr., who has been sent by the Koppers Company to this meeting as its representative."

The President: "Mr. Sperr, I have much pleasure in presenting to you, the representative of the Koppers Company, a John Price Wetherill Medal."

In response Mr. Sperr said: "On behalf of the Koppers Company, I thank you and The Franklin Institute most sincerely."

AWARD OF THE LEVY MEDAL.

To Mr. Leon S. Moisseiff, C. E., New York City, New York. Dr. W. R. Wright, sponsor.

The President called upon Dr. Wright.

Dr. Wright: "Mr. President, upon recommendation of the Committee on Science and the Arts, I have the privilege of presenting to you, to receive a Louis E. Levy Gold Medal, Mr. Leon S. Moisseiff, consulting engineer of New York City.

"This award is made to Mr. Moisseiff for his paper entitled 'The Design, Materials and Erection of the Kill Van Kull Arch' which was published in the JOURNAL OF THE FRANKLIN INSTITUTE in May, 1932.

"When the Levy Medal was established in 1923, it was stated that it should be awarded for papers contributed to the JOURNAL of the Institute descriptive of the author's researches in physical science, or of his engineering achievements, which have added largely to the sum of knowledge or aided greatly the well-being of mankind.

"Those who have read Mr. Moisseiff's paper in the JOURNAL will agree that he has set forth in a clear, literary style, a description of this brilliant piece of engineering. Because of his masterly presentation of the facts, and the importance of the design of this bridge, the committee felt that his paper met fully the requirements stated in the deed of gift of the Levy Medal.

"Mr. President, I present Mr. Leon S. Moisseiff."

The President presented the Medal as follows: "Mr. Moisseiff, in recognition of the high quality of your paper which was published during the past year in the JOURNAL OF THE FRANKLIN INSTITUTE, it gives me much satisfaction to hand to you the Louis Edward Levy Gold Medal."

In accepting the Medal, Mr. Moisseiff said: "Mr. President, I deeply appreciate the honor of the medal given by The Franklin Institute, more so as an adopted son of this country I feel the deepest satisfaction that I have been able to do something toward the progress of engineering."

AWARD OF THE HENDERSON MEDAL.

To Mr. Otho C. Duryea, O. C. Duryea Corporation, New York City, New York. Mr. M. M. Price, sponsor.

The President called upon Mr. Price.

Mr. Price: "Mr. President, the Board of Managers has voted the award of the George R. Henderson Gold Medal to Mr. Otho C. Duryea 'in consideration of the meritorious

railway engineering and the novel feature embodied in the invention of the Duryea Railway Car Cushion Underframe.'

"The use of Mr. Duryea's invention has resulted in the safety of goods in transportation. The officials of railroads using the Duryea Underframe report that damage claims have been reduced greatly.

"I have the honor to present Mr. Otho C. Duryea."

The President presented the Medal as follows: "As President of The Franklin Institute it is my pleasure to present to you the George R. Henderson Medal. In presenting this medal, we honor you by associating your work with that of a great engineer in whose memory this medal is given."

Mr. Duryea replied: "This very great honor conferred on me by The Franklin Institute is very much appreciated."

AWARD OF THE POTTS MEDAL.

To Mr. Igor I. Sikorsky, Sikorsky Aviation Corporation, Bridgeport, Connecticut. Mr. Henry Butler Allen, sponsor.

The President called upon Mr. Allen.

Mr. Allen: "Mr. President, as Chairman of the Sub-Committee of the Committee on Science and the Arts which recommended the award, it is my privilege to present to you, for a Howard N. Potts Medal, Mr. Igor I. Sikorsky, President of the Sikorsky Manufacturing Company.

"Mr. Sikorsky's development of the multimotored airplane began in 1914, when he was in Russia. He came to this country in 1919 and became an American citizen. In the same year he began to study the aviation requirements of the United States.

"Without burdening this meeting with technical details, I may say that Mr. Sikorsky's work has been so successful that his multi-motored planes are now being operated on the great airways of North and South America which have eliminated distance so far as this continent is concerned. In South America the use of these planes has resulted in establishing mail and passenger communication on both the east and the west coasts. In some cases the routes are over much uninhabited country, both forest and mountains, which require the freedom from failure that the multi-motored plane provides.

"Mr. President, I present Mr. Sikorsky."

The President presented the Medal as follows: "Mr. Sikorsky, upon recommendation of our Committee on Science and the Arts, in recognition of the revolutionary work that you have done in design and improvement of heavier-than-air planes, I, as President of The Franklin Institute, have the honor of presenting to you the Howard N. Potts Medal."

Mr. Sikorsky responded: "Mr. President, I certainly am sincerely appreciative of the great honor which has been given to me today and I hope that this will be another great encouragement to me to do as much as I can to further the work of the airways."

Dr. McClenahan announced that it was just twenty years ago this week that Mr. Sikorsky first flew successfully in a multi-motored airplane.

The President: "In recognizing the work of Mr. Sikorsky, I want to say something about the courage of the man. He not only accomplished the original work, but he did something which to my mind showed more 'guts' than anything I have heard. After the revolution in his country, in which he lost everything, he came to this country and the first thing he had to do was to identify himself. He had no blue prints—he had nothing. Everyone knew of his work, but he had to satisfy the people in this country that he really was Sikorsky. That I think was real courage."

AWARD OF THE CRESSON MEDAL.

To Senor Don Juan de la Cierva, London, England. Mr. J. B. Klumpp, sponsor.

The President called upon Mr. Klumpp.

Mr. Klumpp: "Mr. President, I have the honor to sponsor the award of an Elliott Cresson Gold Medal to Senor Don Juan de la Cierva of London, England.

"From a boy Senor Cierva was interested in building model airplanes and when he was seventeen he built the first airplane ever made in Spain, his native country. In 1918, after the collapse of one of his airplanes, he began the study of the possibility of a ship with some kind of free moving wings with a circular motion. After many trials, he succeeded in

developing the autogiro. The first successful flight, was made in January, 1923.

"After having first demonstrated by flight the success of the autogiro principle from elementary theory, Senor Cierva set about to master all phases of its extremely complex engineering and with painstaking study his research resulted in a classified accumulation of data sufficient for autogiro design. This he has set down in a book entitled 'Theory of the Autogiro.'

"It is appropriate to say at this point that the whole history of the autogiro shows little resort to trial and error but rather a continual progression of fundamental conceptions followed by practical proofs, and the gentleman who has honored us by coming from London to be present at these exercises today has himself borne the burden of this development in all its varied phases. That small group of engineers who in recent years have been privileged to work with him are unified in their praise of his continuing leadership.

"Mr. President, in consideration of his original conceptions and inventive ability which has resulted in the creation and development of the autogiro, I present to you Senor Don Juan de la Cierva, to receive the Elliott Cresson Gold Medal."

The President made the award as follows. "Senor Cierva, in the name of The Franklin Institute, I have the honor to present to you the Elliott Cresson Gold Medal. This is given in recognition of the extraordinary ability with which you have applied your knowledge of mathematics and mechanics to the solution of aeronautic problems."

In accepting the award Senor Cierva replied: "Thank you. It is a hard thing for me to make a speech, but I will do my best to say a few words.

"I would like to tell you in a very condensed manner the philosophical idea behind the autogiro. If I were to attempt fully to discuss its principles, it would require elaborate explanations which would only confuse the issue. I think the best thing I can do to give you what I have in mind is to tell you something that happened many years ago in Madrid.

"As Mr. Klumpp has just explained to you, at that time I was experimenting with airplanes. There was an old General who had never seen an airplane at close quarters. He came

out to the airdrome and after he had been shown how the propellor turned and how the plane went through the air, he expressed a desire to make a flight. He climbed into one of the cockpits and the mechanic started the motor. When the pilot was about to take off, the voice of the general was heard, commanding him to fly slowly and very close to the ground! That is one order the pilot did not obey! After the general was off, the officers and people laughed, but I thought the general was right in wanting to fly close to the ground. I thought we should be able to control our flight at low altitudes and at low speeds.

"As you have heard, I designed several airplanes in my early youth. I have the highest admiration for the airplane, but I say precisely that because I have dealt with them, I have seen their limitations. I saw that neither safety as understood by man, nor utility, could be fully achieved. I may be mistaken, but I still think that sustentation in the air and control in the air should be independent of high forward speed.

"When my friend the general wanted to fly near the ground, I asked myself whether there could not be other methods of flight different from the three which were known—the airplane, the lighter-than-air craft and the helicopter. I knew that for many years attempts had been made to solve the helicopter problem. The more I studied the matter the more I came to the conclusion that the secondary difficulties of the helicopter were too difficult to be overcome. Then I asked myself whether that was all; whether there was not some other method of flying, different from those three. I analyzed and I came to the conclusion that there were others but that meant a new type of machine would have to be invented. I established the principle that the flying machine that I was looking for was not going to be dependent upon high forward speed for its lift. I also made a definite condition that these characteristics were to be independent of engine failure. These studies brought me to the idea of the autogiro.

"I am full of faith in the future of the autogiro, and the very great honor which The Franklin Institute gave me will certainly encourage me to bring it to its ultimate perfection.

"I feel that it is an honor for me to have my name associated on this programme with Dr. Wright and Mr. Sikorsky."

The President: "When we see one of Senor Cierva's planes it looks very simple, but all you have to do is to open his book in Mr. Rigling's library and you will see how intricate it is and how great an invention he had really produced. I think Senor Cierva is one of the great, fortunate men of the world. He had an idea, he has carried that idea through to perfection, and through his idea he has won a great friend, Mr. Pitcairn of Philadelphia."

AWARD OF FRANKLIN MEDALS

To Paul Sabatier, D.Sc., Dean of the Faculty of Science, Toulouse University, Toulouse, France. Represented by Monsieur Jules Henry, Counselor of the French Embassy in Washington. Dr. James Barnes, sponsor.

The President called upon the Secretary.

Dr. McClenahan: "The two Franklin Medals have to be presented in absentia. We had hoped greatly to have with us today Professor Sabatier, a great chemist in France, but because of his advanced years he was not able to make the trip. We are happy to have him represented by the Councillor to the French Embassy in Washington, Monsieur Jules Henry. Professor Sabatier has written a letter which we have had translated by some good friends. I will not risk the shock to my French friends of trying to read his letter in French.

"'FACULTY OF SCIENCES,
"'UNIVERSITY OF TOULOUSE

"'TOULOUSE, February 28, 1933

"'*Dear Sir:* You have informed me that The Franklin Institute of the State of Pennsylvania has decided to bestow upon me a Franklin Medal, in recognition of my work. I am extremely honored by this choice which will be for my long career as a chemist a crowning distinction, quite unhoped for. My scientific life which dates especially from the day when I was chosen as assistant by my illustrious master Berthelot (November 1878) has brought me much satisfaction, and I had looked for nothing further, when this Franklin Medal gave me an unexpected and particularly cherished joy. I pray you to express to the members of The Franklin Institute my profound gratitude.

"'I beg you, dear sir, to accept the expression of my most heartfelt appreciation.

"'(Signed) PAUL SABATIER,
"'*Member of The Institute of France.*'"

Dr. McClenahan continued: "We had hoped to have with us today to receive in person a second Franklin Medal the great progenitor of aviation science—Orville Wright. Dr. Wright expected until two or three days ago to be with us. Unfortunately, he is ill and has written this letter which he has asked me to present to you.

"'May 13, 1933.

"'*Dear Dr. McClenahan:* I regret very much to have to tell you that I can not be in Philadelphia next Wednesday to receive from The Franklin Institute the award of the Franklin Medal.

"'I think you already know that as a result of an injury received in my accident at Fort Myer in 1908 I often have severe attacks of sciatica due to the vibration encountered in travel. As long as I avoid travel I am almost entirely free from it. I made a trip to Chicago in February which started trouble. About a month ago, when I thought I was about recovered, I made a trip to Washington. Since that time I have had much difficulty in getting about, and it will be out of the question for me to attempt travel next week. I have waited until today to write you in the hope that I might improve sufficiently to get to Philadelphia, but I now know that that will be impossible.

"'I had looked forward with pleasure to being present on May 17th to receive the award and I regret that this unfortunate condition has arisen

"'Please convey to the Board of Managers of The Franklin Institute an expression of my sincere regret that I can not be present.

"'Sincerely yours,
"'(Signed) ORVILLE WRIGHT.

"Dr. Howard McClenahan, Secretary,
The Franklin Institute,
Philadelphia, Pennsylvania."

The President called upon Dr. Barnes.

Dr. Barnes: "As has just been reported to the meeting by the Secretary of the Institute, the Franklin medalists are unable to be with us today. It is my privilege to present, though I am sorry to say, in absentia, Dr. Paul Sabatier, Dean of the Faculty of Sciences, Toulouse University, Toulouse, France, as the recipient of a Franklin Gold Medal 'in recognition of his numerous and fruitful contributions to the general field of chemistry and especially to organic chemistry, in which he discovered the catalytic activity of finely divided common metals and devised methods for their use in science and industry.' May I present to you, Mr. Jules Henry, Counselor to the French Embassy in Washington, who has been kind enough to come here this afternoon to receive this medal on behalf of his distinguished countryman."

The President then presented the Medal, the Certificate and the Certificate of Honorary Membership, as follows: "Mr. Counselor, by authority of The Franklin Institute, it is my privilege to hand to you for transmission to your distinguished countryman, Dr. Paul Sabatier, the Franklin Gold Medal.

"This award has been made in recognition of his life work as research chemist which has just been described.

"In handing you this medal, I wish to state that it is the highest award in the hands of the Institute to give."

The Counselor made the following acceptance speech:

"When a representative of France comes to Philadelphia, his memory goes back naturally to the period when you achieved your independence, an achievement in which the French people were proud to participate. He is immediately reminded of the close ties your historical metropolis had with his country. He does not forget that here rung the Liberty Bell and the Declaration of Independence was written. In Philadelphia landed the first envoy from the King of France to the newly-born American nation. Later on, many distinguished compatriots of mine, among them Chateaubriand, Joseph Bonaparte and Talleyrand, found your City a most hospitable and interesting one to reside in during the troublesome days of the Revolution. In more recent years, another narrow link was established between my country and your city when Stephen Girard, a Frenchman from Bordeaux, after a

well filled life, bequeathed to every new generation of your young men his wealth and his spirits.

"From Philadelphia embarked legions of Americans who in 1917 and 1918, rushed across the Atlantic to help us in the dark hours of the War.

"But when a Frenchman evokes all those souvenirs, he cannot restrain a natural feeling of emotion in adding to them one which is particularly dear to him: the memory of one of the founders of your country whose name this Institute bears. France shares the admiration of the world for Benjamin Franklin, his works, social, political, scientific or diplomatic. But the long grey-haired inhabitant of Passy, who, upon his arrival in France, so greatly impressed the King and his court and was loved by the people, holds a most unique place in the hearts of my compatriots. You may well understand, then, why a distinction such as the one you are conferring upon Professor Paul Sabatier, will be of such precious value to him. It will not only mean that the works of a long life, entirely devoted to the progress of science, have been appreciated by one of the greatest scientific institutions of the world, where exists such a useful French-American cooperation. For him it will be a symbol. In his name, as well as in the name of the French Ambassador, who is detained in Washington by his duties, it is a great honor to accept the Franklin Gold Medal.

"In honoring Professor Sabatier, The Franklin Institute is adding one more name to the list of eminent Frenchmen whose works and discoveries have greatly contributed to human welfare; Lavoisier, Laplace, Pasteur, Branly, Curie, etc. Many others, although of less fame, distinguished themselves in French-American science.

"*Andre Michaux*, botanist, sent to America by King Louis XVI and who presented to the American Philosophical Society a survey of trees and plants of the Western United States;

"*Volney*, who wrote a "Tableau of the Climate and Soil of the United States";

"*Du Pont de Nemours*, the best pupil of Lavoisier and founder of the chemical industry in America:

"*Brown-Sequard*, who held a Chair at Harvard and Paris and succeeded Claude Bernard at the College de France.

And although their works were not purely scientific, I would be tempted to name *Lesseps*, who conceived the idea of the Panama Canal, and *Lenfant*, who designed the beautiful capital of the United States.

"Today, The Franklin Institute is giving its support to the French American Institute of Science. A new strength will be given to a coöperative work that started during the visit of Lafayette to America, found its expression during the Revolution when French doctors helped fighting the yellow fever epidemics, and covered many lines of science, including engineering and architecture. Big things are not accomplished in one day, especially in science. As Tennyson wrote: 'Science moves but slowly, slowly, creeping on from point to point.'

"An encouragement such as the one Professor Sabatier is receiving today from you cannot fail to facilitate new researches and discoveries from which humanity will derive better health, more comfort, moral as well as material.

"Mr. President, with the regrets of Professor Sabatier not to have been able to come to America for this unique occasion allow me to convey to you and to the Trustees of The Franklin Institute, the expression of his profound gratitude for the award he is receiving."

The President: "Thank you, Monsieur Henry. I think that at this time it is particularly appropriate that these international recognitions of science should take place, when we are all struggling so hard to avoid foolish and unnecessary misunderstandings."

To Orville Wright, Dr. Tech. Sci., D. Eng., D.Sc., LL.D., Dr. James Barnes, sponsor.

The President called upon Dr. Barnes.

Dr. Barnes: "I am honored to have the opportunity to present to you, Dr. Orville Wright, in absentia, who has been awarded the Franklin Medal 'in recognition of the valuable investigations carried out by him and his brother, Wilbur, from which they obtained the first reliable scientific data concerning the principles of flight and the design of airplanes upon which they constructed the first heavier-than-air machine which flew by its own power under human control.'"

The President presented the Medal, the Certificate and

the Certificate of Honorary Membership as follows: "I am going to to ask our good Secretary if he will forward this Medal and the Certificate of Honorary Membership to Dr. Wright. I am going to ask you to authorize him to express our appreciation of his great work and our great regret that he is unable to be with us. Dr. Wright is a doer and not a talker. My family asked him to stay with us on this anticipated visit to Philadelphia. When I questioned whether he would stay with us I was advised, 'Probably not. He is very shy.' When I first heard he was not coming I thought that perhaps we had scared him off. That was not so. He is really ill. No man could be as active as he had been without suffering for it, as well as getting certificates and medals. I think that all students of aviation recognize that these two brothers did something—they revolutionized all mechanical transportation.

"Dr. McClenahan, I ask you to send these to Dr. Wright with our great appreciation. Perhaps you would care to address the meeting?"

Dr. McClenahan: "Mr. President, I promise faithfully to send this medal and these certificates to Dr. Wright, and with great self-sacrifice refrain from making a speech."

The President declared the meeting adjourned.

NOTES FROM THE U. S. BUREAU OF STANDARDS.*

CLAYS FOR SAGGERS.

Saggers are used to protect pottery, tile, abrasive wheels, and other ceramic products from furnace gases, ashes and rapid fluctuations in temperature.

They are box-like containers made as a rule from second or third quality fire clay. The saggers are heated, before being used, at a temperature approximating that at which ceramic ware subsequently placed within them is heated. Such preliminary heating develops the strength necessary for handling them and for carrying the loads to which they may be subjected. They are built strong enough to permit their being stacked upon one another to considerable heights, thereby decreasing the cost of maturing the ware in any individual unit.

Although the average purchaser of clay ware never sees a sagger or realizes its significance, yet he or she must bear its cost when purchasing pottery or other ceramic ware. The loss encountered annually from breakage of saggers and resulting spoiled ware amounts to several millions of dollars. The Bureau has been studying saggers and sagger clays for several years with a view to reducing that loss. It has studied the properties of approximately 50 sagger clays as well as saggers made from those clays. A report which will be published in the Journal of the American Ceramic Society gives the results of tests recently made on a series of sagger bodies together with a chart which it is believed will permit the manufacturer of saggers to predict with a reasonable degree of accuracy the relative length of service which may be expected from saggers made from combinations of clays of which some of the properties have been measured. The properties which were considered are:

(1) Thermal expansion; because of rapid temperature

* Communicated by the Director.

changes to which saggers may be subjected during service it is desirable they should have a low expansion.

(2) Elastic properties; because the greater the rigidity the more difficult for the material to adjust itself to rapid thermal volume changes to which it may be subjected.

(3) Plastic flow at high temperatures; because if a sagger is heavily loaded, the bottom must resist a tendency to bulge and crack.

MEASUREMENT OF MOISTURE EXPANSION.

Twelve specimens of ceramic whiteware were stored for one year in closed containers over water and their moisture expansion determined by direct and indirect methods. The values, by direct measurement, ranged from zero to 0.14 per cent. and did not vary in any case by more than 0.02 per cent. from the values by indirect measurement. This duplicates the conclusion reached with autoclaved specimens (Technical News Bulletin No. 184; August, 1932) that results by the two methods agree within the precision of the direct measurements.

STRESSES IN ENAMELS.

Examination of the coefficients of thermal expansion of enamels and the metals to which they are applied, below the critical temperatures of the enamels, does not yield a comprehensive picture of the stresses created in enamel coatings as they cool subsequent to fusion upon the metal at red heat. The effects upon such stresses which are produced by (1) the rapid change in expansivity of an enamel which occurs at its critical temperature; (2) variations in the rate of cooling of the enamel and in the differential cooling rate between the enamel and the iron; (3) variations in the enamel composition to produce changes in critical and softening temperatures; and (4) variations in the thermal expansivity of enamels, are all being studied by an adaptation of the Schurecht ring-test, originally applied to glazed ceramic bodies. The application of this method has been extended, however, to include observations of the strain-temperature relationship throughout rapid and slow heating and cooling cycles. To this end the changes in width of a

slot cut through a ring-shaped band of iron with enamel applied to one side, are observed during the temperature cycles by means of a telescope. Thus, the movements of a ring towards and away from its unstressed position, which are brought about by differential expansion or contraction between the enamel and the iron, can be closely followed and quantitatively measured. Simultaneously, temperature measurements are made by means of thermocouples spot-welded to the iron under the enamel. Since the expansion curves of the enamels, plotted against temperature, are known from previous determinations, it is possible to correlate the stress changes with given phases in the expansional or contractional paths of the enamels.

VISCOSITY OF OPTICAL GLASS.

Research Paper No. 577 in the July number of the *Bureau of Standards Journal of Research* is a report of an investigation of the viscosities, at high temperatures, of six optical glasses—light barium crown, borosilicate crown, soda-lime, and dense, medium and barium flint glasses. A concentric-cylinder type viscometer, an electrically heated furnace, and porcelain stirrers and crucibles were used. For a limited number of tests the porcelain was protected with platinum.

The driving torque was produced by masses of known weight, and the time required per revolution was measured with a stop-watch automatically started and stopped. Temperatures were determined by means of four platinum to platinum rhodium thermocouples.

Preliminary to work on glass, determinations were made of friction corrections, and certain factors, including the so-called "end effect," were evaluated. The method of producing and testing constancy of rotational speed is described. The classical equation for viscosity, when using an ideal concentric cylinder viscometer, was modified to fit the apparatus used. The paper sets forth methods for the derivation and correction of the miscellaneous factors involved.

Viscosity values were determined for each glass in the temperature range 900° to 1,400° C. (1,652° to 2,552° F.), and

equations are given expressing the mathematical relations of viscosity to temperature. A brief discussion of the viscosity changes resulting from volatilization of the glass and its reaction with the crucibles and stirrers, and of the consequent difficulty in determining the exact composition of a glass at the time the test is made, is included.

EXTRACTION OF LEVULOSE FROM JERUSALEM ARTICHOKES.

During the past 12 months important progress has been made in the Bureau's investigation of the recovery of levulose from Jerusalem artichokes on a semi-commercial scale. This is the first year in which the artichoke juices were extracted solely by means of the battery, and the first year that the new larger battery was employed. The results have demonstrated anew that diffusion juice is far superior to pressed juice as material from which to recover levulose.

Battery Extraction.—A total of about 50 short tons of Jerusalem artichokes was worked, of which about 8½ tons came from Mississippi and the rest from the local fields at Beltsville, Md., and Arlington, Va. The tubers from Beltsville were by far the best, both as to size and the percentage of sugar contained. It was found perfectly feasible to extract 97–98 per cent. of the 14–16 per cent. of reducing sugars which these artichokes contained, and to produce at the same time a diffusion juice which had a density of 14–15 brix. Very little trouble was encountered with the washing of the artichokes, except that certain parts of the machine are nearly worn out and tend to fail at crucial moments. The washer, which was designed and developed by the Bureau, still washes very thoroughly, and when properly adjusted, allows no stones to pass. Small roots and other fibrous parts which were attached to the artichokes, and hence were not removed by the washer, gave a great deal of trouble. They clogged the knives of the small drum slicer and caused the cossettes to be mushy and resistant to the passage of the water and juices in the battery. Otherwise the small slicer functioned very satisfactorily and produced the best grade of chips obtained from artichokes. Owing to the relatively small size of even the largest tubers, however, the artichoke chips cannot com-

pare with good beet chips as material for extraction in a diffusion battery.

Precipitation.—The precipitation experiments were very successful on the whole. Most of the levulate precipitate was filtered on the automatic rotary drum vacuum filters. It seemed impossible to apply sufficient wash water to displace the mother liquor in the cake when the most free-filtering precipitates were obtained. Higher purities were obtained when, owing to the poor quality of the juice, very unsatisfactory precipitates were being produced; that is, when the rate of filtration was small enough to allow adequate washing. The gain in purity may have resulted only partly from the better washing; but at least the results indicate that a clean precipitation is not necessary for the production of good purities—which is contrary to the Bureau's previous assumptions. The last two precipitations of this year's artichoke juices were filtered on the vacuum leaf filter, which has about eleven times the combined filtering area of the two automatic rotary drum vacuum filters used for levulate filtration. This was done partly to speed the work but mainly to study the effect of more extensive washing and to observe the behavior of the material on this type of filter. The residues were filtered on a plate and frame filter press which was used exclusively in some of the Bureau's early work several years ago. Thus there has been obtained certain comparative results with three types of filter on this year's juices. This year, as the result of improvements in the mechanical equipment, it has been found feasible to precipitate more concentrated solutions than had previously been considered practical. The indications are that the losses in waste water are not proportionately higher.

General Results.—Probably the greatest improvements in this year's run, compared with the runs of previous years, can be attributed to the use of diffusion juice exclusively instead of incidentally; to improve technique in the hydrolysis, or "conversion" of the polysaccharides, a result which was made possible by improvements in the mechanical equipment; to the use of lime of much better quality both physically and chemically; to improvements in the equipment for carbonation; and especially to improved methods of treatment im-

mediately following carbonation. There have been discovered the sources of several systematic mechanical losses in the processing, which, though suspected, had been previously unexplained or not demonstrated.

A number of problems remain to be solved in the practice of the levulate precipitation and carbonation, but in the main this part of the process seems to be in a fairly satisfactory state of development. A considerable part of this development has been accomplished during the last year.

CONFERENCE OF STATE UTILITY COMMISSION ENGINEERS.

The eleventh annual conference of State Utility Commission Engineers was held at the Bureau on June 1–3, 1933, inclusive. Twenty-four engineers, representing twelve States, the District of Columbia, and the Province of Ontario, Canada, were present.

The following papers and written discussions were presented:

1. The grounding of electric circuits, A. G. Mott, Railroad Commission, San Francisco, California—discussion by M. G. Lloyd, Bureau of Standards, Washington, D. C.
2. Limitations of voltages in joint use of poles, J. K. Pettengill, Public Service Commission, Lansing, Michigan—discussion by P. L. Holland, Public Service Commission, Baltimore, Md.
3. Contacts of the Airways Division, Department of Commerce, with public utility companies, F. C. Hingsburg, Airways Division, Bureau of Lighthouses, Department of Commerce, Washington, D. C.
4. Grade-crossing classification and protection, E. I. Rudd, Public Utilities Commission, Hartford, Connecticut—discussion by J. G. Hunter, Railroad Commission, San Francisco, California.
5. Determination of billing demand, C. B. Hayden, Public Service Commission, Madison, Wisconsin.
6. Transportation by Motor Vehicle:
   Rules and regulations for transportation by bus, H. C. Eddy, Board of Public Utility Commissioners, Newark, N. J.

Truck and bus regulations, J. Houston Johnston, Public Service Commission, Atlanta, Ga.

The open road, R. H. Nexsen, Public Service Commission, New York, N. Y.

Taxicab regulations, F. A. Sager, Public Utilities Commission, District of Columbia.

Discussions by P. L. Holland, Public Service Commission, Baltimore, Md., and J. P. Wadhams, Public Utilities Commission, Hartford, Conn.

7. Line extension policies, Harry Barker, Public Service Commission, Montpelier, Vermont—discussion by Philander Betts, Board of Public Utility Commissioners, Newark, N. J.
8. Relation of underground corrosion data to pipe-line economies, K. H. Logan, Bureau of Standards, Washington, D. C.—discussion by T. R. Tate, Public Utilities Commission, District of Columbia.
9. Maximum demand meter approvals, R. H. Nexsen, Public Service Commission, New York, N. Y.—discussion by William Black, Public Service Commission, Harrisburg, Pa.

Copies of the papers and discussions are obtainable only from the authors, to whom application should be made.

An Executive Committee to prepare for next year's conference was elected as follows: R. H. Nexsen (N. Y.), Chairman, Harry Barker (Vt.), J. N. Wiley (N. D.), P. L. Holland (Md.), J. C. Boyer (Va.), J. Franklin Meyer, Secretary (Bureau of Standards, Washington, D. C.).

DISCONTINUANCE OF MONTHLY ANNOUNCEMENTS OF NEW PUBLICATIONS.

For several years the Bureau has issued a monthly leaflet listing new publications in its series. Because of the limited funds available for printing, this leaflet was discontinued with the June, 1933 number, Announcement No. 96. Practically the same material will be available in the Technical News Bulletin which contains abstracts of most of the Bureau's publications and also lists by title all papers by members of

the staff in the Bureau's series and in outside journals. All those who have been receiving the monthly announcements are urged to subscribe to the Technical News Bulletin.

DISCONTINUANCE OF COMMERCIAL STANDARDS MONTHLY.

The *Commercial Standards Monthly* was discontinued with the June, 1933 number, which completed Volume 9. Reduction in the Bureau's funds available for printing have made this necessary.

NOTES FROM THE BARTOL RESEARCH FOUNDATION OF THE FRANKLIN INSTITUTE.

APPLICATION OF LIOUVILLE'S THEOREM TO ELECTRON ORBITS IN THE EARTH'S MAGNETIC FIELD.

BY
W. F. G. SWANN,
Director.

It is pointed out that in the application of Liouville's theorem to the problem of cosmic ray intensities, Lemaitre and Vallarta [1] have implicitly taken the electron momentum as that corresponding to a free particle. Calling this momentum p' the particle momentum, we have to realize that Liouville's theorem is usually based upon the Hamiltonian equations in which the momentum p associated with an electron is not the same as p', but is connected with it by the relation $p = p' + eU/c$, where U is the vector potential determining the magnetic field. The Hamiltonian equations are not valid in terms of momenta of the type p', and, it is not, therefore, clear that Liouville's theorem is valid when expressed in terms of these momenta. The object of the paper is to show that a theorem the equivalent of Liouville's theorem is in fact true in terms of the coördinates and the momenta p', so that the ultimate validity of the use of the theorem by Lemaitre and Vallarta is substantiated.

It is to be observed, moreover, that the validity of this extended form of Liouville's theorem is true even in the presence of an electric field.

COSMIC RAY NUCLEAR DISINTEGRATIONS.

BY
W. F. G. SWANN,
Director,
AND C. G. MONTGOMERY,
Research Fellow.

Cosmic ray nuclear disintegrations associated with what are usually termed Hoffman's Stösse have been studied in an

[1] G. Lemaitre and M. S. Vallarta, *Phys. Rev.*, **43**, 87 (1933).

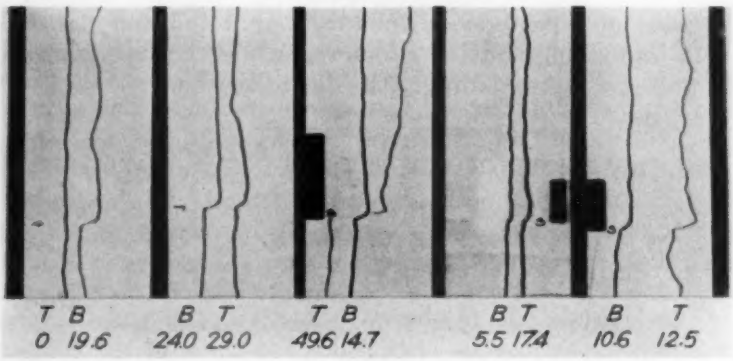
iron vessel 120 cms. high, 90 cms. in diameter, with wall thickness about 17 mm. The vessel was provided with similar independent electrodes in the top and bottom halves, and these halves could be separated by a slab of metal of any desired thickness. The purpose was to study the spurts of ionization in the two halves in the absence of the slab, and then observe it again with the slab, and so deduce from the indications in the two halves information as to the number and character of the nuclear disintegrations occurring in slabs of different materials, and the absorbability of the products of disintegration in the materials.

Each insulated system was connected to an independent FP54 pliotron operating a short period galvanometer. Suitable means for compensation of average ionization current and of fluctuations of applied potential were employed.

In order to provide further evidence as to the reality of the indications, experiments were performed in which a Geiger Mueller counter system, consisting of three sets of counters placed in different positions around the vessel, was used, and arranged so as to record through the amplifier only if rays went through each simultaneously. The two galvanometers recording the spurts of ions reflected vertical lines onto the horizontal slit of the photographic recording apparatus, so that the motions of the lines drew curves on the moving paper. When the counters discharged they lighted a lamp which also illuminated the slit, producing on development a shadow whose starting edge indicated the instant at which the counter discharges took place.

The figure represents records of several of the nuclear disintegrations observed. It will be seen that in most of the examples given simultaneous spurts were observed in the top and bottom halves. The simultaneous discharge of the counters is indicated in three of the examples shown by the lower edge of the shadow *S*. These counter records taken with apparatus entirely dissociated from the ionization apparatus, and produced by effects outside of the vessel demonstrate conclusively, by their correspondence with the spurts of ionization, the reality of those spurts, and relieve them of any suspicion of being caused by accidental phenomena, vibrations and so forth.

At the lower end of each trace is given the number of millions of ions corresponding to the spurt of ions observed on that curve. If one knew the average distribution of the secondary rays which participated in producing any of the above spurts, and if he should assume a number for the ionization per centimeter of path by the rays, he could calculate the number of rays involved in the spurt. The greatest distance which any ray can travel in one of the halves of the vessel is about 150 cms. The gas in the vessel was nitrogen at pressures of the order 100 lbs. per square inch. Hence if using the ion spurt corresponding to 49.6×10^6 ions recorded in the above figure, we divide by 100/14, the pressure in



T—top half of chamber. *B*—bottom half of chamber. Values in millions of ions at 100 lbs./sq. in. of nitrogen.

atmospheres, by 150 cms., the maximum path length, and by 50 ions/cm., which is the order of magnitude of the ions produced per centimeter of path by an electron in nitrogen at atmospheric pressure, we shall obtain the minimum number of rays which can be associated with this nuclear disintegration if these rays are assumed to be of the positive or negative electron type with energies corresponding to those usually associated with cosmic ray phenomena. The number in question comes out as approximately 1000. It of course varies with the spurt considered. However, for all spurts examined it is large. There of course remains the somewhat unlikely possibility of the ionization being caused by a few particles of very much greater ionization per centimeter of

path than that corresponding to electrons. As evidence against this, however, we have the counter data.

Each of the 3 counters had an area of about 250 sq. cm. An examination of the statistics of the problem shows that if in an appreciable fraction of ionic spurts, say 20 per cent. as observed, the counters also record, then the number of rays is probably not considerably less than what would correspond to an average of one ray through each counter set. Taking into account the average distances involved, we are led to the conclusion that the nuclear disintegrations observed corresponded to at least 100 secondaries, and may of course represent many more. The possibility of there being more secondaries than would correspond to the total number of electrons and protons in the disintegrated atom naturally raises interesting considerations regarding the mechanism of the processes accompanying the disintegration.

ON THE NATURE OF THE PRIMARY COSMIC RADIATION.*

BY

W. F. G. SWANN,

Director.

The latitude effects observed by Compton and Clay call for charged particles as responsible for some of the primary radiation. Compton [1] has emphasized the conclusion that the facts of absorption in the atmosphere combined with the existence of directional and latitude effects necessitate the assumption that the particles originate at least several hundred kilometers above the earth's surface in order that with the energy necessary to penetrate the earth's atmosphere they can still experience sufficient bending by the earth's magnetic field to account for the facts. If the charged particles come from distances many times the earth's radius, energies comparable with 10^{10} volts are necessary in order that, in spite of the earth's field, they may reach the earth at latitudes as low as those at which their presence is revealed

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* *Phys. Rev.*, June 1, 1933.

[1] A. H. Compton: "A Geographic Study of Cosmic Rays," *Phys. Rev.*, 43, pp. 357–403, 1933.

by directional and latitude effects. On the other hand, as Millikan has remarked, cloud chamber measurements have given no evidence of charged particles with energies as high as 10^{10} volts; and, it is difficult to suppose that absorption of energy by the atmosphere could reduce a 10^{10} volt particle to an energy of 10^{9} volts, such as represents about the maximum observed in cloud chamber experiments.

Many years ago the writer [2] gave reasons founded upon the classical theory, for supposing that charged particles with sufficiently high energy may be incapable of producing ions on account of the radiationary reaction resulting from the very sharp acceleration which would be necessary were an electron to be ejected from an atom by a charged particle travelling with very nearly the velocity of light, and consequently with a field highly concentrated in its equatorial plane. Recently, he has attacked the problem on a wave mechanical basis, and has obtained an equation representing the radiation reaction terms. At his request the equation was applied to the ionization problem by Dr. A. Bramley [3] and it appeared that on the basis of this equation ionization should become inappreciable at about 10^{10} volts. If we should accept this conclusion we have a possible explanation of the difficulties cited above. We may then suppose that the primary charged particles entering the atmosphere, generate secondaries, which continue in the direction of the primaries and so carry with them the latitude effects and the directional effects fundamentally inherent in the primaries. In other words it is not the action of the magnetic field on the secondaries which is responsible for these effects, but the action of that field on the primaries. On this view the primaries would not show themselves in Geiger counters. Their properties, however, would be exemplified by the behavior of the secondaries as above implied. It is of course to be understood that the secondaries here referred to would in general serve as primaries in the sense of producing other secondaries (or tertiaries) and so on, so that there is no inconsistency in the experiments

[2] W. F. G. Swann: "The Absence of Ionization by Electrons with Speed Comparable with that of Light," *Phil. Mag.*, **47**, pp. 306–319, 1924.

[3] W. F. G. Swann and A. Bramley: "Particle Ionization for Velocities Approaching that of Light," JOUR. OF FRANKLIN INST., **214**, pp. 606–608, 1932.

such as those of Street and Johnson [4] in which evidence is deduced in support of the view that the rays there referred to as primaries are ionizing rays. It would indeed seem that if we accept the arguments of Millikan and Compton together, they practically demand the assumption of *charged* particles of a *non-ionizing* character in line with the ideas expressed above.

A further point bearing upon the matter concerns the fact that the directional effects above referred to have indicated that the incoming primary rays are positively charged. On the view implied in this letter, however, this would not mean that the actual rays which set off the counters are positively charged. These rays might be of either sign, and the crucial experiment would be one in which it was demonstrated that these rays which inherit from the primaries a directional effect appropriate only to a positive charge may, nevertheless, themselves involve rays with charge of either sign. Cloud chamber experiments would afford information on this matter, for, quite apart from the fact that one would have to admit that secondaries will be found in the cloud chamber, one must also admit that if the primary rays exist in the number demonstrated by the directional experiments, and if they are ionizing, they certainly should show themselves in the cloud chamber and should there reveal their true energy and the sign of their charge.

On the view that the real primary rays are non-ionizing, we have yet to admit that they may be capable of causing other atomic transitions such as are associated with the emission of secondaries with high velocity, the initiation of nuclear disintegrations, etc. The relative probability of these various occurrences is a matter whose solution awaits a more complete development of the wave mechanical theory. It may be of interest to point out that if one should accept the views here tentatively suggested, one finds in them an explanation of those peculiar phenomena found by Anderson in the cloud chamber, phenomena in which a secondary appears to be ejected from a piece of lead without the evidence of any primary having entered it.

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[4] J. C. Street and T. H. Johnson: "Concerning the Production of Secondaries by Cosmic Radiation," *Phys. Rev.*, **42**, pp. 142–144, 1932.

THE EQUIVALENCE OF MASS AND ENERGY.*

BY

K. T. BAINBRIDGE,

Research Fellow.

An experimental test of the relationship $\Delta E = C^2\Delta m$ is possible for two modes of nuclear disintegration when the results of recent mass-spectrograph measurements of the masses of the lithium isotopes [1] are considered in conjunction with the disintegration experiments of Cockcroft and Walton [2] and of Lewis, Livingston and Lawrence.[3]

The experiments of Cockcroft and Walton with lithium show that the lithium isotope of mass number seven under bombardment by high energy protons may capture a proton and that the resulting nucleus may disintegrate into two α-particles. The α-particles of range 8.4 cm. which appear for protons of 270 kilo-volts energy have an energy of $8.6_2 \times 10^6$ electron-volts. The gain in energy in the reaction is $16.9_7 \times 10^6$ *e*-volts, an energy equivalent to 0.0182 mass units on the O^{16} scale if $\Delta E = C^2\Delta m$. Taking Aston's values [4] for the mass of helium and hydrogen and the author's value, 7.0146 ± 0.0006 for Li^7, the mass change is $0.018_1 \pm 0.0006$ in the reaction which may be represented as $Li^7 + p \rightarrow 2\alpha$. Within the probable error of the measurements the equivalence of mass and energy is satisfied.

Recently Lewis, Livingston, and Lawrence have reported some remarkable disintegration experiments wherein the H^2 nucleus was the bombarding particle. In the case of lithium, α-particles of two ranges, 8.2 and 14.8 centimeters, were observed as the products of disintegration. The bombarding particles were H^2 nuclei of 1.33×10^6 *e*-volts energy. Extrapolation of the range-energy data for α-particles secured by Rutherford, Ward, and W. B. Lewis [5] yields 12.5×10^6 *e*-volts for the energy of the α-particles of greatest range.

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* July 15, 1933, issue of *Phys. Rev.*

[1] K. T. Bainbridge, *Phys. Rev.*, **44**, July 1, 1933.

[2] J. D. Cockcroft and E. T. S. Walton, *Proc. Roy. Soc.*, **137A**, 229 (1932).

[3] Gilbert N. Lewis, M. Stanley Livingston, and Ernest O. Lawrence, *Bulletin of Am. Phys. Soc.*, **8**, No. 4, 13 (1933).

[4] F. W. Aston, *Proc. Roy. Soc.*, **115A**, 487 (1927).

[5] E. Rutherford, F. A. B. Ward, and W. B. Lewis, *Proc. Roy. Soc.*, **131A**, 684 (1931).

Under the assumption that it is the Li^6 isotope which captures an H^2 nucleus and disintegrates into two α-particles, the process may be indicated as $Li^6 + H^2 \rightarrow 2\alpha$. If one makes the additional assumption that momentum is conserved, the gain in energy in the reaction is 23.7×10^6 e-volts, equivalent to 0.025_5 mass units on the O^{16} scale if $\Delta m = \Delta E/C^2$.

Taking Aston's value for the mass of helium and the writer's values for H^2 and Li^6, the mass change is $0.023_8 \pm 0.0004$ unit. This measured mass change does not agree satisfactorily with the mass equivalent of the energy change in the disintegration. A more strict comparison must wait on values which include the probable error in the determination of the ranges of the α-particles.

June 16, 1933.

CONCERNING THE PRODUCTION OF GROUPS OF SECONDARIES BY THE COSMIC RADIATION.*

BY

J. C. STREET,
Research Fellow,

AND THOMAS H. JOHNSON,
Assistant Director.

The experiments with counters and cloud chambers have shown that the ionization attributed to the cosmic radiation is produced by ionizing corpuscular rays (capable of discharging a counter or of producing a cloud track) of energies ranging from 10^6 to 10^{10} electron volts. At least some of these corpuscular rays are secondaries originating within the surrounding matter, but whether the primary radiation which ejects these secondaries itself consists of ionizing corpuscles or is of a non-ionizing gamma-ray or neutron character has been an unanswered question.

Experiments by Rossi [1] and by the writers [2] have shown that there are frequent groups consisting of two or more divergent ionizing rays which emerge simultaneously from a block of lead, and a similar grouping phenomenon has been observed by those who have worked with cloud chambers. These observations could be explained by any one, or all of

* *Physical Review*, **42**, No. 1, October 1, 1932, pp. 142–144.

[1] B. Rossi, *Phys. Zeits.*, **33**, 304 (1932); *Rend. Lincei*, XV, 734 (1932).

[2] Johnson and Street, *Phys. Rev.*, **40**, 638 (1932).

three postulates. I. The primary may be an ionizing ray which in passing through matter produces other ionizing rays by close collisions with electrons or nuclei. II. The primary may be a non-ionizing ray whose energy is at once transferred by a single nuclear collision to a group of secondary ionizing rays. III. The primary may be a non-ionizing ray whose energy is degraded by the formation of a succession of ionizing rays along an extended path.

To distinguish between these postulates we have made some experiments with the arrangement of three counters and the lead block shown in Fig. 1. If II is the correct hypothesis the presence of the lead block in the position indicated should have only an absorbing effect on the triple coincidence counting rate whereas an increase due to the lead should be noticed if I or III is right. The results in Table I

TABLE I.

Distance *s*.		Total Counting Period.	Total Counts.	Counts per Minute.	Difference Due to Lead.
4 cm.	With lead	2484 min.	136	0.055±0.003	0.015±0.004
	Without lead	3971	159	0.040±0.002	
5 cm.	With lead	3069	132	0.043±0.002	0.014±0.003
	Without lead	2512	73	0.029±0.002	
7 cm.	With lead	5795	194	0.034±0.002	0.018±0.002
	Without lead	3886	63	0.016±0.001	

show an increase due to the lead which is well above the statistical probable error, proving that at least a part of the grouping phenomenon must be accounted for either by postulate I or by III or perhaps by both.

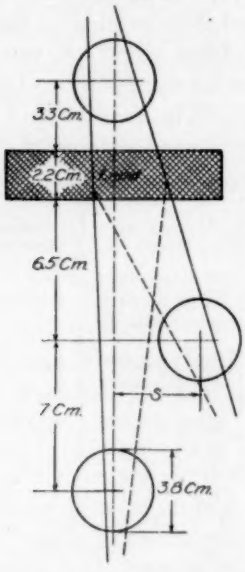
To determine which of these two postulates applies, some experiments with the arrangement of counters and lead blocks shown in Fig. 2 have been carried out. In this case the circuits were arranged for counting the double coincidences between counters 1 and 3 simultaneously with the triple coincidences. Counts were made both with the three lead blocks in position and with all of the lead removed. An explanation [3] of the transition effects discovered by Schindler [4]

[3] T. H. Johnson, *Phys. Rev.*, **41**, 545 (1932).

[4] H. Schindler, *Zeits. f. Physik*, **72**, 625 (1931).

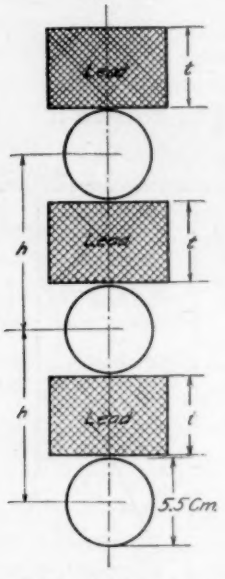
and by Rossi [1] requires that, whether I or III is correct, the secondaries which enter any one of the lead blocks from above shall, for the most part, be absorbed in the thickness of lead used so that the probability that a primary be accompanied by a secondary below a lead block is independent of the condition above. However, most of the secondaries are sufficiently penetrating to pass through all three counters in the absence of the lead. If E is the efficiency of counter 2, the ratio T/D of triple to double counts without the lead is E

FIG. 1.



Effective length of counter 9 cm.

FIG. 2.



Effective length of counter 12 cm.

whereas, with the lead in place, this ratio is EP, where P is the probability of any element of the path of the primary in lead being traversed by at least one ionizing ray (whether it be the primary itself or one of its secondaries). The results obtained are shown in Table II, from which it appears that there is no change in the ratio T/D due to the lead within the limits of error. Hence P is equal to unity. We must conclude, therefore, that the primary ray is either itself an ionizing ray or, if it is a non-ionizing ray, it is always accom-

TABLE II.

Spacing between Counters (h).	Thickness of Lead (l).	Total Counting Period.	Total Double Counts (D).	Total Triple Counts (T).	T/D.
13 cm.	5 cm.	1228 min.	2289	1913	0.84±0.01 with lead
		1072	2389	1954	0.82±0.01 without lead
18 cm.	10 cm.	1993	2074	1731	0.84±0.01 with lead
		1497	2014	1615	0.81±0.01 without lead

panied by at least one of its secondary ionizing rays. This latter possibility must be excluded on other grounds. In the first place, the groups observed in the cloud chamber seldom contain more than two ionizing rays, and in the second place, a comparison of ionization measurements with counter data indicates an average of only two to three ionizing rays per group. These considerations necessitate the conclusion that if the primary is a non-ionizing ray it would be unaccompanied by ionizing secondaries over such a large fraction of its path that P would be less than unity by an easily detectable amount. (If three were the average density of independently formed secondaries 20 per cent. of the path of the primary should be non-ionizing.) We are, therefore, left with the conclusion that all of the groups observed in the first experiment arise from an ionizing primary. This ray, of course, may itself have been produced by a non-ionizing gamma-ray or neutron and, furthermore, it is still possible that a part of the groups observed by other arrangements of counters or in the cloud chamber may arise according to postulate II.

We are indebted to Dr. E. C. Stevenson for his help in arranging the circuits and in recording the data.

COMPARISON OF THE ANGULAR DISTRIBUTIONS OF THE COSMIC RADIATION AT ELEVATIONS 6280 FT. AND 620 FT.*

BY

THOMAS H. JOHNSON,
Assistant Director.

The ray intensity $j(\theta)$ of the cosmic radiation has been studied as a function of the angle θ from the vertical on Mt. Washington, New Hampshire, elevation 6280 ft. and at a base station, elevation 620 ft. The results show a distribution

* *The Physical Review*, 43, No. 5, March 1, 1933.

less concentrated about the vertical at the higher elevation. Unless some *ad hoc* assumption is invoked this result is inconsistent with the intensities and absorption coefficients deduced by Millikan and Cameron as their results would lead to a considerably more concentrated distribution at the upper elevation. Integrating the ray intensities over all angles, the total numbers of rays per cm.^{2} per sec. have been computed at the two elevations. The increase in the number of rays with elevation is less than the known increase in the ionization (Millikan and Cameron). The ratio of numbers of rays per sec. is 1.46 against 1.91 for the ratio of ionizations at the two elevations. The average 6280 ft. ray, therefore, produces 1.31 as many ions per unit path as the 620 ft. ray. This change in ionizing efficiency may perhaps be explained by supposing that the softer rays which are more predominant at the upper level, produce more secondary rays from the walls of the ionization vessel.

Although the angular distribution of the cosmic radiation at a single elevation has been measured by Bernardini,[1] Medicus,[2] Tuwim,[3] Skobelzyn[4] and others, it is believed that the measurements herein reported are the first to show the changes in this distribution with elevation. The relative intensities of the radiation at various angles from the vertical have been measured on Mt. Washington, New Hampshire, elevation 6280 ft. and at a nearby station in Maine, elevation 620 ft., under conditions such that the results at the two elevations may be compared.

On the basis of the simple theory which neglects magnetic effects and scattering, and assumes an isotropic distribution of the radiation at the top of the atmosphere, the intensity, $j_i(\theta)$, of a component of the radiation (defined as the number of rays per unit solid angle per cm.^{2} per sec.) of which the absorption coefficient is μ_i, is a function of the directions of its path through the atmosphere. At a depth h from the top of the atmosphere the intensity at angle θ from the vertical should be given by

$$j_i(\theta) = j_{oi}e^{-\mu_i h \sec\theta}. \qquad (1)$$

The total intensity at angle θ is therefore given by the sum of a series of such terms corresponding to the various components of the primary radiation.

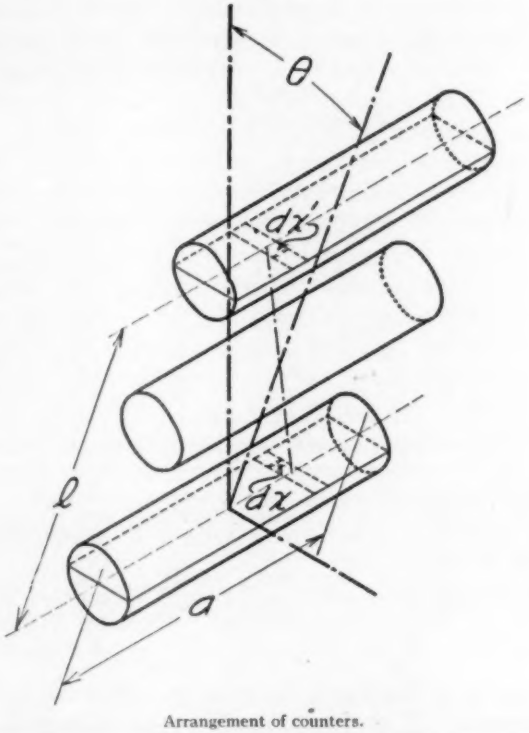
$$j(\theta) = \Sigma j_i(\theta). \qquad (2)$$

[1] G. Bernardini, *Nature*, **129**, 578 (1932).
[2] G. Medicus, *Zeits. f. Physik*, **74**, 350 (1932).
[3] L. Tuwim, *Berlin Berichte*, **19**, 91, 360 (1931).
[4] D. Skobelzyn, *Comptes Rendus*, **194**, 118 (1932).

Since secondary rays are also included in the measurements, this theory cannot be rigorously exact for undoubtedly the secondaries do not always follow the directions of the primary rays which produce them, and some large angle scattering of the primary rays may also take place. Therefore, the measured distribution must be less concentrated about the vertical than that represented by (2). For this reason it would be futile to deduce values of j_{oi} and μ_i from the present data, although this would be possible on the basis of the above simple assumptions.

The apparatus consisted of three Geiger-Mueller counters mounted in line (Fig. 1) and connected for recording triple coincidences.[5] The counters were cylindrical, 12.7 cm. long

FIG. 1.



Arrangement of counters.

[5] T. H. Johnson and J. C. Street, J. FRANKLIN INST., March, 1933.

and 3.8 cm. in diameter and were spaced 10 cm. between adjacent axes. The cross-sectional area of the sensitive volume was determined by comparing the coincidence counting rate of these counters with that of a pair of counters specially constructed to eliminate end corrections. The resulting sensitive areas, assumed rectangular, were 8.8 cm. by 3.8 cm. This method for making end corrections is not entirely satisfactory as it does not take account of the relative efficiencies of the two sets of counters. For the purpose at hand, however, it is sufficiently accurate, since the length of the sensitive volume enters the computations only through a small correction term.

Observations were made with the line of the counters pointing towards the magnetic south at various inclinations from the vertical. Readings were taken and the inclination changed at the end of approximately three hour intervals. A summary of the results is contained in Table I, which

TABLE I.

Summary of Observations.

θ.	n.	T.	C.	r.	R.	R'.	f.	j.
Mt. Washington, elevation 6280′, mean barometer 590.5 mm.								
0°	6	1100	2789	2.54	0.046	0.033	0.00650	0.0165
20°	9	1487	3256	2.19	.024	.026	.00648	.0142
30°	5	909	1662	1.83	.031	.030	.00644	.0118
40°	7	1185	1635	1.38	.024	.028	.00642	.0089
60°	6	987	532	0.54	.013	.016	.00636	.0034
80°	5	1106	120	0.11	.002	.006	.00632	.00069
90°	3	664	28	0.04	.003	.005	.00632	.00025
Base station, elevation 620′, mean barometer 748 mm.								
0°	8	1637	2940	1.80	0.015	0.022		0.0117
20°	5	1058	1570	1.49	.019	.025		.00966
30°	5	1198	1381	1.15	.016	.021		.00741
40°	5	1032	892	0.86	.008	.019		.00552
60°	4	729	251	0.35	.013	.015		.0022
80°	4	707	33	0.05	.005	.005		.00032
90°	1	181	8	0.04		.01		.00025

gives, for each inclination, the number of runs n, the total time in minutes T, the total number of counts C, and the number of counts per minute r. The probable error of r as

estimated from the residuals is given in the table under R.

$$R = 0.6745[\Sigma v^2/(n-1)n]^{\frac{1}{2}}.$$

If no instrumental variations are present and the cosmic rays are of constant intensity, the variations should be just those arising from the statistical fluctuations. The probable error should, therefore, depend only on the number of counts C, and its value should be $R' = 0.6745 \times C^{\frac{1}{2}}/T$. The values of R' are also included in the table and the agreement of R and R' is an indication of the reliability of the result. This agreement is good in every case with the possible exception of the Mt. Washington 0° value and, even there, the discrepancy is not great enough to definitely indicate trouble. For cases in which R is less than R' the latter is the more accurate value.

Since the solid angle subtended by the counters was small, the counting rates, $r(\theta)$, are, to a fairly close approximation, proportional to the intensity $j(\theta)$, θ referring to the inclination of the line of centers of the counters. (Fig. 1.) There is, however, a small geometrical correction having a maximum variation with θ of 3 per cent., which can be determined by an integration extending over the areas of the two extreme counters. Over the small range of inclinations of lines joining points on the axes of the two extreme counters, it has been assumed, for the purpose of the integration, that the intensity varies as the square of the cosine of the inclination from the vertical, this law being approximately correct empirically. Furthermore, if it be assumed that the variation of j is linear over the short range of angle subtended by the diameters of the extreme counters, integration along this dimension becomes unnecessary and the total number of rays per second passing through all three counters when pointing in the vertical direction ($\theta = 0$) is given by

$$r(0) = j(0)d^2l^4 \int_{-a'/2}^{+a'/2} \int_{-a/2}^{+a/2} dx'dx/[(x-x')^2 + l^2]^3$$
$$= j(0)/F(0).$$

Here d is the diameter, a' and a are the lengths of the two extreme counters, and l is the distance between the extreme axes. Since the inclinations of lines joining axial points of

the two extreme counters vary less and less with increasing inclination of the line joining the centers, the factor in the integral which takes account of the change of intensity of the radiation over the range of angles subtended by lengths of the counters becomes equal to unity for θ equal to $\pi/2$ and

$$r(\pi/2) = j(\pi/2)d^2l^2 \int_{-a'/2}^{+a'/2} \int_{-a/2}^{+a/2} \frac{dx'dx}{[(x - x')^2 + l^2]^2}$$
$$= j(\pi/2)/F(\pi/2).$$

For intermediate angles it is easy to show that

$$j(\theta) = r(\theta)F(\pi/2)(1 + \epsilon \cos^2 \theta) = r(\theta)f(\theta),$$

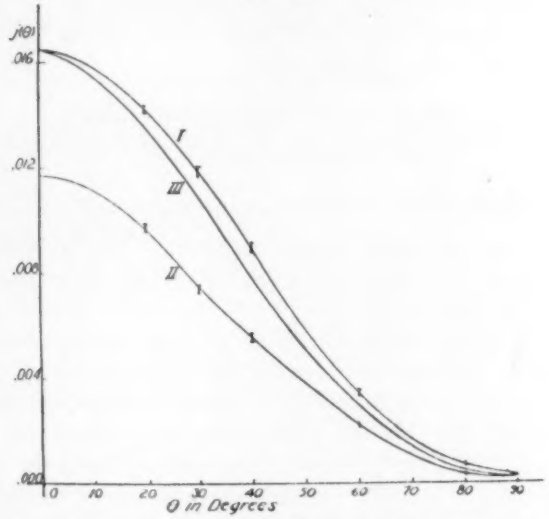
where $\epsilon = F(0)/F(\pi/2) - 1$. The factors $f(\theta)$ are given in the tables as also are the corresponding values of $j(\theta)$. These values of $j(\theta)$ are still not strictly equal to the intensities, for the efficiency of the counters has not been measured and the end corrections are not accurately known. They are, however, proportional to the intensities at a single elevation. Before comparing the results at the two elevations it is necessary to investigate possible changes in the efficiency with the counting rate of the individual counters, since this changed from 100 per min. at the base station to 200 per min. on Mt. Washington.

For the purpose of this investigation the triple coincidence counting rate has been measured in the laboratory with the individual counting rates controlled by the proximity of a tube of radium emanation. These measurements consisted of a series of 34 determinations of the triple counting rates with the individual counting rates alternated between 85 per min. and 197 per min. The measurements satisfied the $R = R'$ criterion and they showed the change in coincidence counting rate to be less than the probable error (2.5 per cent.). Therefore, no further correction to the j values listed in the table need be applied to make them comparable at the two elevations.

In Fig. 2, $j(\theta)$ has been plotted against θ. Curve I represents the Mt. Washington values, curve II the values at the base station and curve III has been computed by multiplying II by a constant factor. A comparison of I and

III shows that the distribution is broader at the higher elevations. This is contrary to the change one would have expected on the basis of the intensities and absorption coefficients deduced by Millikan and Cameron [6] as they would have led, on the assumptions of the simple theory, to a considerably sharper distribution at the higher elevation. The distribution at the lower elevation, however, is in good agreement with their intensities and absorption coefficients

FIG. 2.



Intensity of cosmic rays as function of inclination to the vertical. I. Elevation 6280 ft. II. Elevation 620 ft. III. II × 1.41.

and the discrepancy at the higher elevation arises from the very intense soft component which they have deduced.

Another point of apparent inconsistency between the ionization-intensity measurements and the ray-intensity measurements is the difference in their respective variations with elevation.

The ray-intensity, J, defined as the total number of rays per cm.^{2} per second from all directions, is given by

$$J = 2\pi \int_0^{\pi/2} j(\theta) \sin \theta \cos \theta d\theta.$$

[6] R. A. Millikan and C. H. Cameron, *Phys. Rev.*, **37**, 235 (1931).

The integration has been performed graphically with the results

$$J_{6280} = 0.024 \text{ rays per cm.}^2 \text{ per sec.}$$
$$J_{620} = 0.016 \text{ " " " " "}$$
$$J_{6280} : J_{620} = 1.46.$$

The ionization-intensities I, defined as the number of ions per c.c. per sec. produced by the cosmic radiation in an unshielded electroscope were computed for the barometric pressures experienced on Mt. Washington and at the base station by interpolation from the values given by Millikan and Cameron.[6] The values are:

$$I_{6280} = 70.0 \text{ ions per c.c. per sec.}$$
$$I_{620} = 36.7 \text{ " " " " "}$$
$$I_{6280} : I_{620} = 1.91.$$

It appears, therefore, that the average ray at 6280 ft. has (1.91 ÷ 1.46) 1.31 times the ionizing efficiency of the average ray at 620 ft. elevation.

As a possible interpretation of the latter discrepancy between the ionization-intensity and ray-intensity measurements, it is suggested that the softer radiation which predominates at the higher elevations may have the greater tendency to produce groups of secondary rays in the walls of the ionization vessel. If this interpretation is correct the experiments, such as that of Schindler,[7] would be expected to show greater "übergangseffekte" at higher elevations and it would imply that the cosmic-ray ionization as measured in a closed vessel would depend upon the wall thickness and the nuclear constitution of the wall material in a way which varies with the elevation. The unexpected broader distribution at the higher elevation may have its explanation in a more predominant large angle scattering on the part of the soft rays.

The calculations are, of course, based upon the assumption of a uniform azimuthal distribution and the results would be affected by any departures from uniformity such as are

[7] H. Schindler, *Zeits. f. Physik*, **72**, 625 (1931).

indicated by the measurements on Mt. Washington made by the writer in collaboration with J. C. Street.[8]

It is a pleasure to acknowledge the coöperation of J. C. Street in developing and building the apparatus and in taking some of the readings. The writer is also indebted to H. N. Teague and Myron E. Witham of the Mt. Washington Club for the use of Camden Cottage in which the measurements on Mt. Washington were made.

ANGULAR DISTRIBUTION OF LOW ENERGY COSMIC RADIATION AND INTERPRETATION OF ANGULAR DISTRIBUTION CURVES.*

BY

THOMAS H. JOHNSON,
Assistant Director,

AND E. C. STEVENSON,
Research Fellow.

One of us has made an experimental determination of the angular distribution of the cosmic radiation at two different elevations,[1] but an interpretation of the data on the assumption of straight line paths through the atmosphere was not advocated since it was uncertain to what extent the distribution might have been affected by divergences between the paths of primary and secondary rays and by scattering in their passage through the atmosphere. Since it is known that the soft rays suffer the greater deviations [2] we have measured the angular distribution of the radiation which is stopped by 3.8 cm. of lead in order to determine the extent to which such scattering affects the angular distribution. At every 15 degree interval between the vertical and horizontal measurements of the coincidence counting rates of three G.M. counters [3] were made alternately with and without a block of lead inserted just above the lower counter.

Each counting rate was the average of fifteen or more independent data, an analysis of which showed that the

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[8] T. H. Johnson and J. C. Street, Minutes of the Atlantic City Meeting of the Am. Phys. Soc., December, 1932.

* *The Physical Review*, 43, No. 7, April 1, 1933.

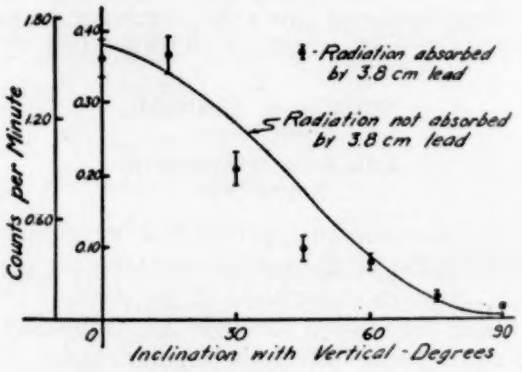
[1] T. H. Johnson, *Phys. Rev.*, 43, 307 (1933).

[2] C. D. Anderson, *Phys. Rev.*, 43, 381 (1933).

[3] T. H. Johnson and J. C. Street, J. FRANK. INST., 215, 239 (1933).

fluctuations agreed with those to be expected statistically and for which the standard deviation is the quotient of the square root of the total number of counts by the total time. Two such complete runs taken several weeks apart were in good agreement. The differences in counting rates with their probable errors as calculated by combining both sets of data are shown by the experimental points in Fig. 1. For

FIG. 1.



comparison the continuous line represents the distribution of the rays which penetrated the lead plotted to a reduced scale. Although there is an indication that a small fraction of the soft rays, equal to about fifty per cent. of the horizontal rays, has been scattered through large angles, the majority are fully as concentrated about the vertical as are the penetrating rays, and we may conclude that the distribution of the total radiation is not appreciably broadened by scattering. We may, therefore, interpret the angular distribution of the total radiation on the basis of straight line paths through the atmosphere.

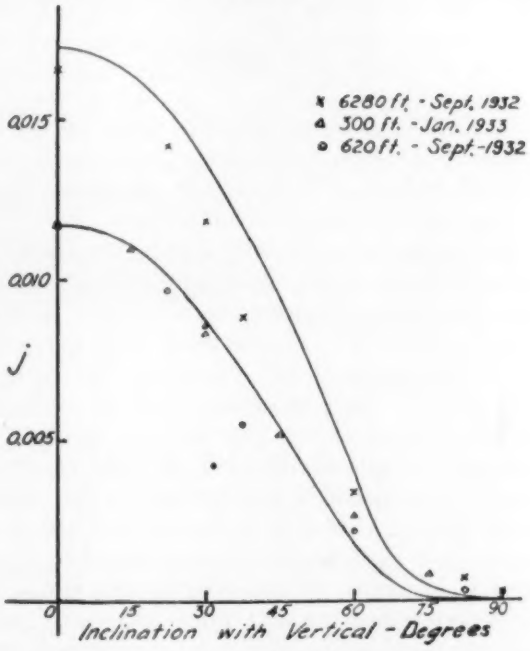
Assuming a simple exponential law the theoretical distribution is:

$$j(\theta) = j_0 e^{-\mu h \sec \theta}.$$

In Fig. 2 are plotted three sets of experimental data showing the distributions on Mt. Washington, New Hampshire, at a base station near Mt. Washington, and on the roof of the Bartol Laboratory. The theoretical distributions for sea

level and the top of Mt. Washington for $\mu = 0.18$ per meter of water are represented by the continuous curves. The agreement between the observed and calculated distributions at sea level is remarkably good and the absorption coefficient is in good agreement with the value 0.16 deduced by Millikan and Cameron [4] from the portion of their ionization-depth

FIG. 2.



Distribution of total radiation. Points are experimental; solid curves are computed for $\mu = 0.18$ per meter of water for elevations sea level and 6280 ft.

curve adjoining sea level. For the Mt. Washington elevation there would seem to be a discrepancy between the observed intensities and those calculated from $\mu = 0.18$. There is, however, some uncertainty in the relative intensities as measured at the two different elevations arising from a change in the efficiency of the counters with counting rate. Although the change in efficiency was measured [1] the uncertainty of

[4] Millikan and Cameron, *Phys. Rev.*, **37**, 235 (1931).

the measurement is sufficient to account for this apparent discrepancy. However, if the absorption coefficient increases with elevation as rapidly as indicated by the data of Millikan and Cameron, unaccountable differences appear between calculated and observed intensities both as regards angular distribution and relative values.

COMPARISON OF THE MASSES OF H^2 AND HELIUM.*

BY

K. T. BAINBRIDGE,

Research Fellow.

The mass of H^2 has been measured from twelve spectra as 2.01363 ± 0.00004 referred to helium [1] as 4.00216 and 2.01363 ± 0.00008 referred to O^{16} when the probable error in the helium-oxygen ratio is taken into account.

Each spectrum included two doublets, $HeH^{1+} - H_2{}^2H^{1+}$ and $HeH^{2+} - H_3{}^{2+}$ for which the separations are identical and equal to the difference between the masses of neutral helium and neutral diatomic H^2. A spectrum then gives two determinations of the mass of H^2 one from the comparison of HeH^{1+} with $H_2{}^2H^{1+}$ and another from the comparison of HeH^{2+} with $H_3{}^{2+}$.

The measured positions of the HeH^1 and HeH^2 molecular ions provided the dispersion measurements for these spectra. The mass of H^1 was taken as 1.007775 and the mass of He as 4.00216, as their ratio had been determined from a previous investigation.[2] An error of one part in one thousand in the dispersion determination could only introduce an error of one part in 200 thousand in the determination of the mass of $H_2{}^2H^{1+}$ or an error of one part in 240 thousand in the mass of $H_3{}^{2+}$.

This insensitiveness to possible errors in the dispersion constant is an ordinary advantage inherent in the measurement of close doublets. In addition it can be shown that the use of two related doublets, as in the present determina-

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* This appeared as a "Letter to the Editor" in the July 1, 1933, issue of *The Physical Review*, which contains a reproduction of some of the spectra used in the comparison of the masses of H^2 and He.

[1] F. W. Aston, *Proc. Roy. Soc.*, **115A**, 487 (1927).

[2] K. T. Bainbridge, *Phys. Rev.*, **43**, 103 (1933).

tion, greatly simplifies the detection and elimination of small systematic errors. Also corrections for non-linearity of the mass scale appear only in the fifth decimal place if a single mass determination of H^2 is derived from the average of the two values obtained from the "five" doublet and from the "six" doublet. The probable error in the evaluation of the mass of H^2 from twelve spectra was calculated on the basis of twelve average determinations, not twenty-four single values. Moreover it can be shown that the mass of H^2 is evaluated in terms of He alone and the mass determination of H^2 is insensitive to whatever value is selected for H^1 in the range $\pm$ 0.1 per cent. from the figure 1.007775. Similarly, in obtaining the dispersion from the separation of the HeH^1 and HeH^2 lines, the final mass determination of H^2 is not sensitive to the value of H^2 used to obtain the mass of HeH^2.

The writer is greatly indebted to Professor G. N. Lewis who very generously gave the writer some "heavy water" containing $H^2 : H^1$ in the approximate abundance range of 1 : 2 to 2 : 3.

June 14, 1933.

THE MASSES OF THE LITHIUM ISOTOPES.*

BY

K. T. BAINBRIDGE,
Research Fellow.

The mass of Li^6 referred to O^{16} was measured as 6.0145 $\pm$ 0.0003 from five spectra by comparison with H_3^2 by the "doublet" method. The mass of Li^7 was measured from the same spectra as 7.0146 $\pm$ 0.0006. The lithium ions were obtained from a heated spiral of flat tungsten strip coated with spodumene. Simultaneously a discharge was run in hydrogen containing a high concentration of the heavy isotope. The hydrogen had been put at the disposal of the writer through the kindness of Professor G. N. Lewis.

The mass of Li^{7+} was measured in reference to that of H_3^{2+} by a less direct and less accurate method than was

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* This note appeared as a "Letter to the Editor" in the July 1, 1933, issue of the *Physical Review*, which contains a reproduction of one of the spectra used in the determination of the masses of Li^6 and Li^7.

possible in the case of Li^6. If the mass-scale were strictly linear the mass of Li^7 could be obtained by direct extrapolation. Owing to the divergence of the mass-scale from absolute linearity it is necessary to apply a small negative correction to the mass secured on the basis of a linear mass-scale. This correction is easily found and was known from the dispersion data secured from earlier plates. The probable error introduced by this corrected extrapolation may conservatively be taken as $\pm$ 0.0003 mass unit and it has been included in the probable error given in the first paragraph.

The mass ratio of the isotopes of lithium secured in this investigation does not agree with the value from the band spectrum analysis [1] but it is in good accord with the ratio from Costa's earlier mass-spectrograph measurements.[2]

The author is indebted to Dr. W. F. G. Swann for his interest and helpful discussions. The writer wishes to express his appreciation of the generous gift by Professor G. N. Lewis of the fractionated hydrogen used in this work.

June 14, 1933.

SPACE DENSITY OF COSMIC RAY PARTICLES.*

BY

W. F. G. SWANN,

Director.

A portion of the primary cosmic radiation entering our earth exists in the form of charged particles. If it exists in this form in interstellar space, its existence necessitates a certain volume density of particle radiation there. The object of the present note is to show that this density is large enough to insure that if the particles were all of one sign, there would exist differences of potential of enormous amounts between points in space separated by distances which are relatively small on an astronomical scale.

Suppose $\alpha ds d\omega$ is the number of cosmic ray particles crossing the element of area ds within a solid angle $d\omega$, whose

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[1] Andrew McKellar, *Phys. Rev.*, 43, 215 (1933). F. A. Jenkins and Andrew McKellar, *Bulletin of the American Phys. Soc.*, 8, No. 4, 23 (1933).

[2] J. L. Costa, *Ann. de Phys.*, 4, 425 (1925).

* This appeared as a "Letter to the Editor" of the *Phys. Rev.*, July 15, 1933.

axis is perpendicular to ds. Then the number of rays crossing ds within the solid angle Sin $\theta d\theta d\varphi$ is α Cos θ Sin $\theta d\theta d\varphi ds$, where θ is measured from the normal to ds. The number of these to be found at any instant within the cylindrical element of volume $dsdx$, of length dx perpendicular to ds is α Cos θ Sin $\theta d\theta d\varphi dxds/c$ Cos θ, where c is the velocity of light. The total space density (number of particles per c.c.) contributed by the particles from all directions, including both sides of ds is

$$D = \frac{2\alpha}{c}\int_0^{2\pi} d\varphi \int_0^{\pi/2} \text{Sin } \theta d\theta = \frac{4\pi\alpha}{c},$$

a result analogous to the well-known corresponding formula in heat radiation.

If the particles all have a charge e, the volume density ρ of electricity which they constitute is

$$\rho = 4\pi\alpha e/c.$$

At any point in interstellar space let us draw a sphere with center O and radius R. The field at any point inside the sphere, and so the potential difference between the center and any point P on the boundary is composed of a part resulting from the electricity outside the sphere, and a part resulting from that inside. The difference of potential V between P and O caused by the latter is

$$V = \frac{4}{3}\pi\rho^{-}\int_0^{R} -\frac{r^3}{r^2}dr = \frac{8\pi^2 e\alpha R^2}{3c}.$$

Now if the potential difference between P and O is reduced below V by the field caused by the charge outside the sphere, the corresponding difference of potential will be increased by a practically equal amount in the case of the difference of potential between O and a point Q diametrically opposite to P. In case one has any doubts on this point, imagine the sphere R surrounded by a concentric sphere S of radius large compared with R. The electricity between S and R produces no field inside R, and the field resulting from the more distant electricity outside S produces a field, which is sensibly uniform inside R.[1] The result of these considerations is to the effect

[1] We wish to avoid any vague statements based upon implications that O is the "center" of an infinite distribution of charge.

that there will be points on R which differ in potential from the center O by an amount comparable with V.

Now if we take for α a value no larger than that corresponding to the earth's surface for cosmic rays, i.e. $\alpha = 0.7 \times 10^{-2}$; and if we express V in volts and replace R by the value R_0 measured in light years, we have

$$V = \frac{300 \times 8\pi^2 \times 4.7 \times 10^{-10} \times 0.7 \times 10^{-2} \times (3 \times 10^{10} \times 365 \times 86{,}400)^2 R_0^2}{3 \times 3 \times 10^{10}},$$

$$V = 7.7 \times 10^{17} R_0^2 \text{ volts.}$$

Thus V amounts to about 7.7×10^{17} volts for $R_0 = 1$ light year. Since α should probably be taken as larger than the above value for a point outside the atmosphere,[2] the above value of V certainly represents a lower limit. A sphere one light year in radius is a relatively small sphere as astronomy counts distances. Hence, we see that the assumption of particles of only one sign in sufficient amount to account for cosmic ray intensities would lead to conclusions prohibited by the possibilities of nature. Of course the presence of charges of both signs would alleviate the difficulty without the necessity of assuming that both signs of charge had energy sufficient for cosmic rays. Such a situation would presumably prevail if positive and negative particles were created in space or were released from atoms existing there. Any attempt to picture particles of one sign as emanating from the stars or other heavenly bodies would result in a condition in which the particles would become bound to their respective stars as a sort of space charge unless both signs of charged particles were emitted.

In conclusion it may be remarked that the author's recent suggestion [3] as to the possibility of the emission of high energy charged particles by the heavenly bodies is by no means limited to particles of one sign.

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[2] It will be recalled that in line with the conclusions of Lemaitre and Vallarta [G. Lemaitre and M. S. Vallarta, *Physical Review*, **43**, 87 (1933)] the intensity at the earth's surface should be equal to that at infinity except, of course, for absorption.

[3] W. F. G. Swann, *Phys. Rev.*, **43**, 217 (1933).

THE CONTINUOUS BETA-RAY SPECTRUM.*

BY

ARTHUR BRAMLEY,
Assistant to the Director.

The idea that mass and energy are equivalent has been of the greatest importance in helping to unravel the processes occurring in disintegration problems. Although this concept has been tested in many different ways, the continuous beta-ray spectrum has thrown some doubt on its validity when applied to the changes undergone by an electron in the formation of a beta-particle. In this process, the electron has certainly suffered a radical change in its properties, a change which must have been accompanied by just as great a readjustment of the stresses (non-Maxwellian in origin) which give the electron its identity. The question then arises whether or not the energy associated with these non-electromagnetic stresses has an equivalent mass. However, we should expect the law of the conservation of energy to be always valid.

Kaluza and O. Klein have developed a geometrical interpretation of the equation of motion of charged particles. This has been extended by Flint [1] by the introduction of the idea of Weyl that the length l of a vector A depends on the value of the electromagnetic field. He derives an expression for the length

$$l^2 = y_{mn}A^mA^n = l_0^2 \exp\left[-(2\pi i/h)\int mcdx^5\right]$$

which varies periodically with a frequency mc/h. Schrödinger in a paper on the motion of a free particle shows that an internal oscillation of this same frequency is present.

Both these investigations are based, however, on the supposition that the properties of the electron are fixed. In the incipience of a beta-particle in radioactive decay, on the other hand, the electron's properties are in a state of flux. For example, the rest mass seems to change from practically zero to its final value as the electron leaves the nucleus. In such a case, the mass m will be a function of the coördinates of the path by which the electron leaves and the length of a

* This appeared in the *Physical Review*, **43**, No. 9, 1933, page 767.

[1] Flint, *Proc. Roy. Soc.*, A131.

vector associated with the path will depend on the value of the integral $\int mcdx$, whose final value depends on the path of integration.

In the formation of a beta-particle from the nuclear electron, the amount of energy required to build up the non-electromagnetic stresses may not be independent of the way in which these stresses arise. The variation of the electromagnetic energy of the beta-particle would thus be accompanied by a corresponding variation of the energy of the nuclear stresses but if this latter does not manifest itself as electromagnetic mass we would have no means of detecting it. In any case in which the rest mass of the electron changes we might not expect the law of the conservation of mass to apply. The tentative suggestion, that for the same rest mass of the nucleus the energy of the nuclear stresses may be different depending on the method of building up these stresses, must be decided by further work.

These ideas are based on the supposition that the masses of all the nuclei of the same constituents have the same mass—we are not here considering the small difference which might arise from the nuclear entities not having the same configuration and hence not the same mass in the same way that in the atomic system the energy levels of the atom in the ground state may differ by a small amount.

THE AZIMUTHAL ASYMMETRY OF THE COSMIC RADIATION AT SWARTHMORE.*

BY

THOMAS H. JOHNSON,
Assistant Director,

AND E. C. STEVENSON,
Research Fellow.

In order to reach the surface of the earth an electrically charged particle of external origin must overcome two barriers, the magnetic field and the atmosphere. Since the atmosphere is of uniform depth over the entire surface of the earth the threshold energy which a ray must possess in order to penetrate this barrier is independent of latitude but the threshold energy which is required for the ray to penetrate the magnetic field varies from about 10^{10} volts, in the case of

* Letter to the Editor of *Physical Review*.

electrons at the equator, to zero energy at the poles. The theory by Störmer [1] and by Lemaitre and Vallarta [2] of the effect of the magnetic field on the electrically charged component of the cosmic radiation shows further that an asymmetry is to be found in the directional distribution of those rays whose energy is just above the magnetic threshold and this asymmetry should be observable in equatorial latitudes where the magnetic threshold energy is greater than the threshold determined by atmospheric absorption.

The latitude for which the magnetic threshold is equal to the atmospheric threshold for sea level, as determined from Compton's [3] world survey, appears to be at about 50° geomagnetic latitude. Clay's [4] data is in close agreement but indicates that rays of the threshold energy corresponding to higher latitudes are still able to penetrate the atmosphere. Within 50° or more from the equator, therefore, the predicted asymmetry in the directional distribution should be observable at sea level.

This asymmetry was sought by Rossi [5] in 1931 at Florence, Italy, magnetic latitude 44° N., and by Johnson and Street [6] in June 1932, in Swarthmore, geomagnetic latitude 51°. Both of these attempts failed to reveal any difference between the eastern and western intensities, but in neither case were the probable errors of the result well established. In September 1932, on Mt. Washington, N. H., geomagnetic latitude 57°, barometer 59 cms., Johnson and Street [7] found the east intensity at a zenith distance of 30° to be six per cent. lower than that of the west. This difference was three times the probable error as computed from the total number of counts, and although the number of determinations was insufficient for an independent check on this value of the probable error, yet it was known that, as a rule, the fluctuations of that particular set of coincidence counters were not greater than the

[1] *Wien Harms Handbuch für Experimental Physik*, XXV, 418.
[2] Lemaitre and Vallarta, *Phys. Rev.*, **43**, 87 (1933).
[3] A. H. Compton, *Phys. Rev.*, **43**, 387, 1933.
[4] J. Clay, *Proc. Amsterdam Acad.*, **31**, 1091, 1928.
[5] B. Rossi, *Nuovo Cimento*, **8**, Mar., 1931.
[6] Johnson and Street, *Phys. Rev.*, **41**, 690, 1932.
[7] Johnson, JOUR. FRANK. INST., **214**, 689, 1932. Johnson and Street, *Phys. Rev.*, **43**, 381, 1933.

statistical. It was, therefore, exceedingly improbable that the Mt. Washington results could be explained other than as the effect of the earth's field on a positively charged component of the primary cosmic radiation. In March 1933, the measurements were repeated by one of us [8] in the more favorable latitude of Mexico City confirming the results of the Mt. Washington studies, and a few days later at the same location with independent apparatus the result was again confirmed by Alvarez,[9] the eastern intensity in Mexico being about 10 per cent. lower than that of the west at a zenith distance of 45°, or 50°, with smaller differences at other angles.

Although the previous Swarthmore results had shown no E.-W. differences [10] the latitude intensity measurements and the Mt. Washington results would indicate that an effect should be detectable at the latitude of Swarthmore. Consequently we have made a redetermination of the east-west differences at various zenith distances with an accuracy which now shows a positive effect, the east intensity being 3 per cent. lower than that of the west at 30° from the zenith with smaller differences at other zenith distances. By obtaining a large number of determinations extending over prolonged counting periods, the probable error has been reduced to about 1/4 of the difference so that the result may be regarded as definite. The combined data for various zenith distances is contained in table I and the east-west differences are represented graphically in Fig. 1. The experimental arrangement and order of observations were the same as were used in the Mexico studies. Following our usual custom * the table of data contains the total time of observation T, in each position, the total number of counts C, the average counting rate C/T and the number n of individual determinations of the counting

[8] Johnson, Minutes of the Washington Meeting of the Am. Phys. Soc., April, 1933. Johnson, JOUR. FRANK. INST., June, 1933. Johnson, *Phys. Rev.*, 43, 834, 1933.

[9] Alvarez and Compton, *Phys. Rev.*, 43, 835, 1933.

[10] S. A. Korff has also found no E.-W. difference greater than 4 per cent. at Pasadena. Minutes of the Salt Lake City meeting of the American Physical Society, June, 1933.

* We are indebted to M. A. Tuve, A. G. McNish and W. E. Deming for a discussion of the statistical problem.

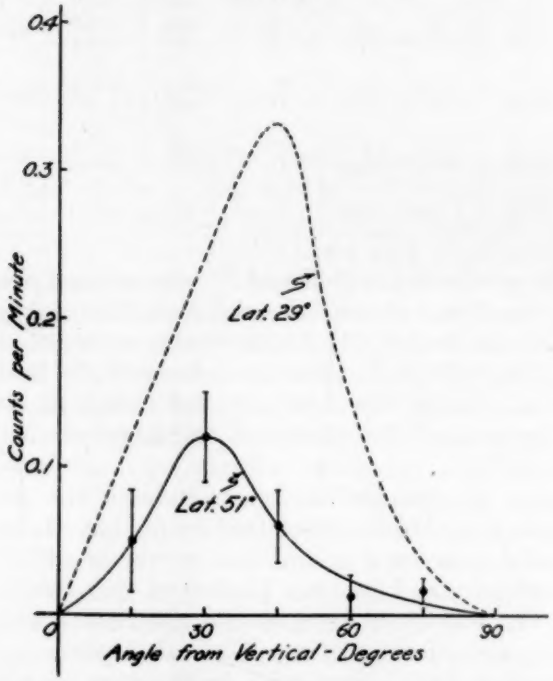
Angle from Vertical.	Total Time T (Min.).	Total Counts C.	Counting Rate C/T.	Number of Data n.	Probable Error from Residuals R.	Probable Error from Number of Counts R'.	West-East Difference.
0°	5721	20,113	5.40	37	0.024	0.026	0
15° East	4418	22,109	5.00	33	0.028	0.023	0.05 ±0.034
West	5565	28,123	5.05	34	0.018	0.020	
30° East	4109	15,202	3.70	37	0.019	0.020	0.12 ±0.030
West	3755	14,367	3.82	37	0.022	0.022	
45° East	4520	10,851	2.40	35	0.016	0.016	0.06 ±0.024
West	5526	13,579	2.46	35	0.018	0.014	
60° East	5538	6,330	1.143	26	0.014	0.010	0.012±0.016
West	5522	6,380	1.155	28	0.010	0.010	
75° East	6815	2,630	0.386	28	0.007	0.005	0.016±0.009
West	6550	2,632	0.402	27	0.005	0.005	
90°	2896	294	0.102	12	0.004	0.004	0

rate. The probable errors R and R' as computed respectively from the dispersion of the data and from the total number of counts are also included. The probable errors of the E.-W. differences as indicated in the last column of the table and by the vertical lines in the figure are calculated as the square root of the sums of the squares of the larger of the probable errors R or R'.

In order to compare the magnitude of the asymmetry here with that in Mexico the latter results have been reduced to the same scale by a comparison of the sensitivity of the two instruments and are thus plotted as the dotted curve of Fig. 1. The difference between the two curves is significant in showing the effect of atmospheric absorption on the rays which produce the asymmetries at the respective latitudes. The rays which are involved in the east-west differences at the higher latitude are obviously softer than those at the lower latitude for the maximum difference appears at 30° from the zenith at latitude 51° compared with 45° or 50° from the zenith at latitude 29°. The softer threshold rays at the higher latitude are thus able to penetrate the atmosphere only at angles closer to the zenith. The difference in the magnitudes of the effects at the two latitudes is perhaps also

indicative of a difference in the rates of absorption of the two radiations though differences of this type could also be explained by the assignment of arbitrary energy distributions. In this connection it is also to be remarked that the range of energies involved in the asymmetry is shorter, according to the theory, at the higher latitude, and this may partially account for the smaller observed effect. Differences in the two curves may also be accounted for by the fact that the data

FIG. 1.



were obtained at two different altitudes. Some unpublished results at sea level in Vera Cruz, Mexico, however, show that by far the greater part of the changes in magnitude and distribution of the E.-W. differences are to be accounted for by the "variation in latitude rather than by the difference in elevation."

These studies are being continued in Panama and Peru through the financial support of the Carnegie Institution of Washington and the Bartol Research Foundation.

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BOOK REVIEWS.

OTHER WORLDS. By Edwin Lincoln Moseley, A.M., Head of the Department of Biology, Ohio State Normal College. xi—231 pages, 55 illustrations, 13 × 19 cms., cloth. New York, D. Appleton & Co., 1933. Price $2.00.

In this little book, the latest member of the publisher's "New World of Science Series," edited by Watson Davis, director of Science Service, the author starts with the closest of astronomical bodies, the meteors, and works from them out to the super-galaxies. Unfortunately, it must be said that it is not up to the high standards of previous books in the same series, which have been written by such eminent authorities in their respective fields as Paul R. Heyl, Theodore Koppanyi and D. T. MacDougal.

The general plan of the book is a good one, but it is marred by a number of errors. It is rather misleading to say, as the author does on page 39, that "comets often do not develop their tails until they come close to the Sun," for none do, and some not, even then. On page 49 it is stated that meteors "would hit much harder" on the Moon than on the Earth "because their speed would not be checked by air resistance." This completely ignores the fact, so clearly pointed out by F. R. Moulton, that a meteoric mass of the size required to produce a large crater would not be appreciably retarded by atmospheric resistance.

It is highly doubtful that Phobos, inner satellite of Mars, would appear from the surface of that planet "as large as the Moon appears to us," as stated on page 114. The weight of astronomical opinion favors a diameter of about ten miles for Phobos, in which case, when viewed in the zenith from the surface of Mars, it would only look a third as large and a twenty-fifth as bright as our Moon.

While it is correct, as stated on page 121, that the fifth satellite of Jupiter "was found in 1892 by Barnard while he was looking through the big refracting telescope" of the Lick Observatory, it is not true that the three others discovered at the same institution "were found later on photographs taken with a camera attached to the same telescope." Two of these were photographed by Perrine with the 36-inch Crossley reflector, and the third by Nicholson with the same instrument. Though the four large satellites were discovered by Galileo in 1610, as stated on the same page, it is not true that this was "the first year that any telescope was used to look at celestial objects," or, as is repeated on page 154, that Galileo "was the first man to use a telescope" for astronomical purposes. It has been satisfactorily shown, in the opinion of this reviewer, that in 1609 Simon

Marius used a telescope and did observe the inner satellites of Jupiter, though apparently he did not realize their true nature, for which discovery the Italian must be credited. It is hardly true, as we read on page 128, that the rings of Saturn can ever be seen "with prism binoculars," unless they are much more powerful than those owned by most of us.

Though the illustrations have been fairly well selected, many of them are very poorly reproduced. This is particularly true of Figs. 48, 50 and 54, made from G. W. Ritchey's magnificent photographs of the Pleiades, the Orion nebula and the Andromeda nebula. The two star maps, Figs. 41 and 42, showing the northern sky on a winter evening and the southern sky in summer, are poor. Many of the constellations are distorted beyond recognition, and it is hard to see how the Great Dipper managed to get into the southern sky!

The book is nicely bound, printed on good paper and provided with an index. Perhaps if a future edition is required, the errors may be corrected.

JAMES STOKLEY

QUALITATIVE CHEMICAL ANALYSIS, CERTAIN PRINCIPLES AND METHODS USED IN IDENTIFYING INORGANIC SUBSTANCES TOGETHER WITH A SYSTEMATIC SURVEY OF THE CHEMISTRY OF THESE MATERIALS, by Roy K. McAlpine, Ph.D. and Byron A. Soule, Sc.D., University of Michigan. Based upon the text by A. B. Prescott and O. C. Johnson, 696 pages, tables, 15.5 × 23.5 cms. New York, D. Van Nostrand Company, Inc., 1933. Price $4.50.

This treatise is described on its title page as "Certain Principles and Methods Used in Identifying Inorganic Substances together with a Systematic Survey of the Chemistry of These Materials." It is based upon the classic text of A. B. Prescott and O. C. Johnson. The book is divided into four parts devoted, respectively, to the principles of analytical chemistry, the metals, the non-metals, and systematic examinations. The treatment of the principles is essentially from the viewpoint of physical chemistry, including discussions of such subjects as mass action, reversible reactions, theory of ionization, common ion effect, dilution effects, solubility product, electrolytes dissociating into more than two ions, and the delicacy of reactions. Under the metals, six groups are recognized; and some attention is also paid to the rare elements including rhenium, germanium, the platinum metals, and the rarer metals of the iron, zinc, and alkali groups. The common metals (cations) receive adequate consideration. The part devoted to non-metals pays attention to the common inorganic acids (anions), several of the rarer inorganic acids (such as the thionic acids, chlorous acid, and periodic acid), several organic acids (acetic, oxalic, and cyanic acids), and a number of elements and compounds. This part concludes with the analysis of a solution for acid radicals. The part on systematic examinations considers the removal of organic substances and the examination and analysis of solids (dry unknowns). The book is provided with an appendix of fifty pages, which supplies information on the preparation of reagents, outlines of analytic procedures, solubilities, the balancing of equations, reduction potentials, hydrogen-ion concentration and pH, and the periodic system. This book maintains the high standards set by its predecessor, and will be a welcome addition to any chemical library.

J. S. HEPBURN.

SPARKS FROM THE ELECTRODE, by C. L. Mantell, Ph.D., Consulting Engineer, Pratt Institute, 127 pages, illustrations, 12.5 × 19 cms. Baltimore, The Williams & Wilkins Company, 1933. Price $1.00.

"Sparks from the Electrode" having followed a lengthy treatise on a highly specialized chemical subject, comes like dessert after the heavier difficultly digestible courses. The volume deals with the industries born of the union of electricity and chemistry—the field of high amperages and low voltages, relatively speaking. "The story of their development is for the most part unknown, being confined as it were, almost entirely to the close circle of workers in the field." The tales are worthy of treatment by a modern Grimm or Anderson and bear the attribute of absolute reality.

To many people Niagara Falls is no more than a lot of water falling over a high cliff and they hold the attitude of "After all, how can it be helped." However, the dwellers in that community cannot sustain life by merely feasting their eyes upon the scene. They are fully aware and duly thankful that the Falls has been harnessed to the extent that one and one-half million horsepower are constantly available as a result of which Niagara has become the first center of electrochemical industries.

In the chapter on the March of Progress the author goes back to the year 1807 when Sir Humphry Davy demonstrated to a distinguished gathering a number of chemical reactions caused by the electric current. From that time to the present every decade marked a number of discoveries which directly or indirectly contributed toward this thoroughly modernized industry of gigantic proportions. Under the caption "What Price Purity," the writer takes the reader on an extremely interesting sight-seeing trip through a copper refinery. Here the electric current is utilized for the production of copper 99.95 per cent. pure. From the copper refining slimes, silver to the extent of 95 per cent. may be obtained; 1 to 3 per cent. will be gold and smaller amounts of platinum and palladium may be separated from the residue.

"Mining Metals with Water" tells of the successful working of low grade ores by chemically dissolving out the metal with sulfuric acid. In the case of copper ore, the solution of copper sulfate, is electrolyzed and copper of 99.96 per cent. purity is plated out. A similar process has been adopted for preparing extremely pure zinc from ores that are difficult to smelt. The residues from the leaching of zinc ore are treated for the recovery of cadmium in a pure form and the refiner may go even further and work up the cadmium residues for the indium that is in them.

"New Surfaces for Old" is a historical survey of the development of electroplating. "Twentieth Century Metals" tells the ever-fresh story of electrolytic process for producing aluminum. Processes of a similar nature are now used for producing magnesium, calcium, sodium, lithium and newest of all, beryllium.

"Nature Loses" describes a remarkable instance of a good idea "gone wrong." Acheson in an attempt to produce a form of crystalline carbon unwittingly succeeded in making silicon carbide called carborundum because at first it was believed to be a compound of carbon and corundum. Then, to top off his first "fortunate failure," Acheson had a carborundum furnace overheat a few years later and found that instead of silicon carbide, a greatly improved type of graphite remained in the furnace.

In the "Electrochemical Kitchen" will be found huge electric furnaces for which the production of a wide variety of ferro-alloys is all in the day's work. The rediscovery of calcium carbide may be listed as another lucky accident. A mixture of limestone and cake was heated in an electric furnace in an attempt to produce metallic calcium. None was forthcoming and the cooled charge was thrown out into the yard. Shortly after a passing smoker tossed a lighted match upon the heap during a heavy rainstorm and thus was calcium carbide, progenitor of acetylene, discovered.

Batteries—primary, dry cells, storage—are current examples of the close union between chemistry and electricity. As a dry cell runs down so does its container, a zinc cup, dissolve to form the chemical zinc chloride. Chemical compounds break down and reform as the storage battery is charged and discharged.

In a great number of cases corrosion may be correctly classed as electrolytic in nature. It was the skillful reversal of this process by Fink and Eldridge that has made possible the restoration of antique bronzes which have been badly corroded. . . . Chlorine, the almost universal bleach and disinfectant, owes its complete availability and low cost to the electrolytic dissociation of sodium chloride brines.

"Sparks from the Electrode" is the most recent member of the Century of Progress Series and the author has ably outlined the progress of electrochemical processes since the time of Davy. The final chapter draws an interesting contrast between the tiny furnace with its miniature carbon electrodes first displayed at the Royal Society to the enormous furnaces of today using electrodes eight feet wide, twenty inches thick and six feet long for conducting currents of the order of 100,000 amperes.

T. K. CLEVELAND.

PHYSIOLOGICAL EFFECTS OF RADIANT ENERGY, by Henry Laurens, Ph.D., Professor of Physiology in the Tulane University School of Medicine, New Orleans. American Chemical Society Monograph No. 62. 610 pages, 15 × 23 cm. The Chemical Catalog Company, New York. 1933. Price $6.00.

Dr. Laurens' efforts in the compilation of this volume are notable and they have come none too soon. The work has been one primarily of gathering all the material in the field, not the writing of a text book. A great and exacting task has been accomplished in bringing together and detailing such a vast amount of experimental work, both good and bad, old and new, and frequently conflicting.

Plenty of surprises await anyone who may open this volume, layman or scientist, and thereby it becomes endowed with a considerable measure of fascination. Not that it is easy reading—far from it, in fact except perhaps for glasses the reader will need nothing so much as a well rounded medical vocabulary. Witness the following description of the process of acquiring sunburn, nominated as classic in that to the uninitiated it describes nothing, to the doctor, everything:

"The vessels become dilated and the epithelial layer infiltrated with serum and abnormally thickened and darkened. Following intense irradiation there is serofibrinous and often hemorrhagic exudation. If the irradiation is severe enough the collagenous tissue becomes swollen and homogeneous, the epithelium swollen and loosened, infiltrated and blistered, the loss of continuity occurring at the

boundary between the granular and horny layers. If the irradiation is quite intense, thrombi form in the vessels of the cutis. Irradiation of the ear of a rabbit with a powerful carbon arc lamp produces degeneration and necrosis of the epithelium, blister formation, stasis and fibrinous exudation to the surface and tissue spaces. In addition there is a relatively slight white cell infiltration, mainly of lymphocytes, with plasma cell formation. Finally a marked production of fibroblasts and the formation of new vessels and regenerative processes in the epithelium. The hair follicles and sebaceous glands break down and are regenerated from the epithelium."

No one escapes the effects of radiation, however, and the ordinary reader will be continually surprised at the many and unsuspected ways in which it may either heal or harm in sickness or in health. The influences run from spring fever to coagulation of the proteins of the eye, from treatment of severe sunburn by more radiation to alteration of the blood pressure. Easterners may be glad to learn that throughout the great central portion of California "in spite of the high degree of solar energy, the deficiency in the ultraviolet portion is so great that there is commonly a mild case of rickets among children who are not given cod liver oil or treatments with artificial light during the summer months."

The reviewer suspects that some of the biggest surprises awaiting medical and research men in this field will take the form of citations of unknown work which frequently will prove embarrassing to their present points of view.

If any criticism is to be made, this reviewer would say that the volume appears more formidable than necessary, a matter which might have been relieved to some extent by greater subdivision of the subject matter and by more extended use of diagrams and tables in the interests of condensation, comparison and aid to the memory. Some sections are very long and extremely detailed with data, notably that on "Physical Characteristics of Solar and Artificial Radiation," comprising 34 pages. There still remains a considerable search for one who would quickly put his finger on a certain aspect of a problem despite a reasonably good index.

Seventeen chapters comprise the contents. These cover the physics of radiant energy, its effects on the skin, on wounds and on some skin diseases, on the eye, on the circulatory system, on blood, on metabolism, on body temperature, respiration and blood reaction, on tuberculosis and other particular diseases, and on bacteria, toxins, antitoxins, proteins, ferments, etc. Separate chapters are given to photodynamics or optical sensitization and to mode of action of radiant energy on physiological and pathological processes.

There ought to be use for a more practical book or text which would bring together and coordinate better many sound conclusions that can be drawn from this great assemblage of data.

LESLIE R. BACON.

PUBLICATIONS RECEIVED.

The Therapeutic Agents of the Quinole Group. Cincophen, plasmoquine, nupercaine, quinine and acridine dyes. The relation between their chemical constitution and pharmacologic action, by W. F. von Oettigen, M.D., Ph.D. 301 pages,

15.5 × 23.5 cms. American Chemical Society Monograph Series. New York, Chemical Catalog Company, 1933. Price $6.00.

Steam, Air, and Gas Power, by William H. Severns, and Howard E. Degler, 480 pages, tables, illustrations, 15.7 × 23.7 cms. New York, John Wiley & Sons, Inc., London, Chapman & Hall, Ltd., 1933. Price $4.00.

National Advisory Committee for Aeronautics, Eighteenth Annual Report, 1932, including Technical Reports nos. 401 to 440, 746 pages, tables, illustrations, 23.5 × 30 cms. Washington, Government Printing Office, 1933. Price $2.50.

Canada Department of Mines, Investigations in Ceramics and Road Materials, 1930 and 1931, 175 pages, tables, diagrams, plates, 16.5 × 24.5. Ottawa, King's Printer, 1933.

Royal Institution of Great Britain, Weekly Evening Meetings: March 3, 1933, The New Polar Province, by Sir Douglas Mawson, 18 pages, plates, maps, 14 × 21.5 cms. March 24, 1933, London Passenger Transport, by The Right Hon. Lord Ashfield, 43 pages, illustrations, 14 × 21.5 cms. March 31, 1933, The Nitrogen Hunger of the World, by Prof. Sir Frederick Keeble, 24 pages, 14 × 21.5 cms. April 28, 1933, The Muscular Movements of Fishes, by James Gray, M.C., Sc.D., 32 pages, plates, 14 × 21.5 cms. London, Institution, 1933.

The Method of Dimensions, by Alfred W. Porter, 78 pages, tables, diagrams, 11 × 17.5 cms. London, Methuen & Co., Ltd., New York, E. P. Dutton & Co., Inc., 1933. Price $.85.

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The Louis E. Levy Medal (Gold Medal).—This medal is awarded to the author of a paper of especial merit, published in the JOURNAL OF THE FRANKLIN INSTITUTE, preference being given to one describing the author's experimental and theoretical researches in a subject of fundamental importance.

The Walton Clark Medal (Gold Medal).—This medal is awarded to the "author of the most notable advance in knowledge or improvement in apparatus, or in method concerning the science or the art of gas manufacture or distribution or utilization in the production of illumination, or of heat, or of power."

The Certificate of Merit.—A Certificate of Merit is awarded to persons adjudged worthy thereof for meritorious inventions, discoveries or improvements in physical processes or devices.

The Boyden Premium.—Uriah A. Boyden, Esq., of Boston, Mass., has deposited with The Franklin Institute the sum of one thousand dollars, to be awarded as premium to "any resident of North America who shall determine by experiment whether all rays of light and other physical rays are or are not transmitted with the same velocity."

For further information relating to these awards apply to the Secretary of the Institute.